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Richard L. Garwin
Cold Fusion Panel
July 11-12, 1989

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Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

July 12, 1989

To: Cold Fusion Panel

Enclosed is the Draft Interim Report. Please submit your comments by COB July 21 to John Huizenga (FAX No. (716) 473-6889) with a copy to Dave Goodwin (FAX No. (301) 353-5079).

A hard copy of the draft will follow in the regular mail.

John Huizenga will send you in several days a schedule of future Panel activities. Among these are two meetings, one at Chicago O'Hare Airport on October 13 and a final meeting on October 30-31. Please note these dates on your calendars.

William Woodard

Enclosure

4 #645

DRAFT

INTERIM REPORT OF THE COLD FUSION PANEL TO THE ENERGY RESEARCH ADVISORY BOARD

THIS IS A PRELIMINARY DRAFT THAT HAS NOT YET BEEN SEEN BY ALL PANEL MEMBERS. IT WILL BE SENT TO ALL PANEL MEMBERS FOR THEIR COMMENTS. AFTER THEIR COMMENTS ARE INCORPORATED, THE PANEL'S REPORT WILL BE SUBMITTED TO THE FULL ERAB. THE FULL ERAB WILL REVIEW THE PANEL'S REPORT FOR POSSIBLE MODIFICATION OR REVISION AND APPROVAL PRIOR TO SUBMISSION TO THE SECRETARY.

INTRODUCTION

As a result of the startling announcements in March 1989 by Utah scientists claiming the attainment of cold fusion, the Secretary of Energy requested (see Appendix A) that the Energy Research Advisory Board (ERAB) convene a panel (see Appendix B) to assess the possibility of cold fusion. The panel meetings and schedule of laboratory visits are summarized in Appendix C.

Since the above announcement, many laboratories worldwide have initiated research in cold fusion. In the United States, a major effort has been undertaken to search for cold fusion by a large number of research groups at industry, university, and national laboratories. Unfortunately, at the present time, the reports from different laboratories are quite divergent. Some laboratories claim excess power production attributed to cold fusion, usually for intermittent periods and for various periods of time but with no supporting evidence for the production of commensurate quantities of fusion products. Other laboratories find no measurable excess power production and no measurable high levels of fusion products. Some laboratories attribute the discrepancies to inaccuracies in measurements, others to non-reproducibility of a new and not understood process. Tritium levels above normal have been reported in some cells following electrolysis but not in others. Neutrons near background have been reported in some D_2O electrolysis and pressurized D_2 gas experiments, but at levels 10^{12} below the amounts required to explain the experiments claiming excess power.

In the past 8 weeks the Panel or subgroups thereof have participated in the Workshop on Cold Fusion in Santa Fe, have visited the laboratories listed in Appendix C, have studied the open literature and numerous privately distributed reports, and have participated in many discussions.

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GENERAL CONCLUSIONS

Although the Panel's task is not yet completed, the Panel finds that the experiments reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion. Indeed, evidence for the discovery of a new nuclear process termed cold fusion is not persuasive. Hence, no special programs to establish cold fusion research centers or to support new efforts to find cold fusion are justified at the present time.

However, there remain unresolved issues and scientifically interesting questions stemming from reported cold fusion efforts. Some of these are relevant to the mission of DOE and should be handled by carefully focused and cooperative efforts within current programs by normal mechanisms for project selection.

The reports of excess heat and fusion products are assessed in separate sections. Preliminary recommendations are summarized in the final section.

CALORIMETRY AND EXCESS HEAT

The claim for electrochemically charged palladium cells as prospective energy sources rests on reports of "excess heat" (or, more precisely, excess power) that cannot be accounted for in the thermal balance normally applied to water electrolysis. Among the issues the Panel addressed in site visits were whether the power levels themselves are being accurately measured and whether the reactions being considered in these cells are, in fact, satisfying the chemical assumptions made. These heat measurements have been done with calorimetry varied as to technique and to levels of precision and accuracy. In most cases, calorimetric effects attributable to excess heat are very small and the calorimetric measurements are difficult and subject to subtle errors arising from various experimental problems.

For the purposes of this report, the calorimetry is usefully differentiated as to whether the D_2 and O_2 gases are allowed to exit the cell completely unreacted or are intentionally catalytically recombined to regenerate D_2O and to recover the corresponding heat. In the case of open cells, where the gases are assumed to be vented without reaction, any output power (as heat) greater than the electrical input power minus the power equivalent of the D_2O formation enthalpy [1.527 V x I (cell current)] is considered excess, a result reported by several groups. In closed cells with total recombination (and with a deuterium-charged Pd electrode), the total electrical power in and total heat power out would normally balance (as for Pt and Pd electrodes in light water). At present no experimenters who have performed calorimetry with closed cells under strict recombination conditions have reported any excess heat. Another important point is that most of the reported measurements with open cells are actually power measurements, and the data have not conclusively demonstrated that the total amount of energy produced (as heat and chemical energy) exceeds the total electrical energy input.

Since the claimed excess heats have, in most cases, been of a magnitude significantly less than the 1.527 V x I factor itself, issues of calibration,

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reliability, and support of the assumptions of zero recombination are especially critical. The Panel's site visits have identified experimental uncertainties, e.g., nonlinearities of the calibration in power output vs. temperature, time dependence of calibration, and doubtful accuracy of data acquisition relative to the magnitude of the effects asserted. Even in laboratories that report excess heat, this effect, under apparently identical conditions, is often not reproducible. In none of our visits to the different sites did we see an operating cell that was actually producing excess heat. So far, we have seen no experimental results that are sufficiently free of ambiguities and calibration problems to make us confident that the steady production of excess heat has been observed. However, there are reports of sporadic temperature "excursions" or "bursts" that apparently represent power outputs significantly larger than the input power. These events cannot be attributed to problems with accuracy or calibration alone and are presently not understood. In general, the calorimetry to date does not persuasively demonstrate the production of excess heat, but the bursts will require evaluation in the Panel's final report.

FUSION PRODUCTS

Since deuterium fusion necessarily yields fusion products (neutrons, protons, tritium, ^{3}He, ^{4}He, gamma rays), it is essential to establish the presence of such products in any claim of fusion. Each watt of power must be accompanied qualitatively by 10^{12} particles per second. This makes product detection by far the most sensitive method to search for fusion. Results to date on fusion products are summarized in the following paragraphs.

Neutrons are an established signature of the well studied d+d fusion reaction. Although many experimenters report no neutrons, some report as many as 1 neutron per second. If confirmed, this rate would be of some scientific interest (even if not indicative of cold fusion). This rate is so far below the 10^{12} neutrons per second required for 1 watt that it is of no interest as a practical energy source.

Numerous experimenters have sought tritium production in electrochemical cells and have found no excess tritium. One group reports finding up to 10^{14} tritium atoms (neglecting losses to the gas phase) in each of several cells with Pd cathodes and Ni anodes. Some of these experimenters report neutrons produced from similar electrochemical cells, but at a rate of about one neutron per second. If the tritium were a result of deuterium fusion, the rate of neutron production should be comparable and thus some 10^{10} times greater than reported.

Another important fusion signature is ^{3}He which should be detectable within a cathode after operated at fusion power levels of watts. It has been postulated that the cold fusion reaction might conceivably proceed predominately by the production of ^{4}He and thermal energy. None of the researchers to date, including those reporting the production of heat, have reported ^{3}He or ^{4}He above the detectable level of 10^9 atoms. One watt-hour of energy corresponds to more than 10^{15} atoms.

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Low level cold fusion in geologic processes has been proposed to cause high $^3He/^4He$ ratios and tritium abundances associated with volcanoes. Several laboratories are currently attempting to detect volcanic tritium.

INTERIM RECOMMENDATIONS

1. The Panel recommends that the cold fusion research efforts in the area of heat production focus primarily on confirming or disproving reports of excess heat. Emphasis should be placed on calorimetry with closed systems and total gas recombination, use of alternative calorimetric methods, reasonably well characterized materials, exchange of "promising" electrodes between groups, and careful estimation of systematic and random errors. Cooperative experiments are encouraged to resolve some of the claims and counterclaims in calorimetry. Such experiments should be pursued at a limited number of laboratories and supported at a modest level on the basis of competitive proposals. At the present time, the panel recommends against any significant expenditures to establish cold fusion research centers or to support new efforts to find cold fusion.

2. A shortcoming of most experiments reporting excess heat is that they are not accompanied in the same cell by simultaneous monitoring for equivalent fusion products. If the excess heat is to be attributed to fusion, such a claim should be supported by measurements of fusion products at commensurate levels.

3. Experiments designed to check the reported production of excess tritium in electrolytic cells are desirable.

4. Experiments reporting fusion products (e.g., neutrons) at a very low level, if confirmed, are of scientific interest but have no apparent applications to the production of useful energy. Continued support of such experiments at modest levels is justified, provided the proposals for such research are evaluated in comparison with other DOE research proposals. In view of the difficulty of these experiments, collaborative efforts are encouraged to maximize the detection efficiencies and to minimize the background.

DEPARTMENT OF ENERGY
WASHINGTON, DC 20585

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(Name/Office Symbol/Telephone No.)

FROM: BILL WOODARD SECRETARY, COLD FUSION PANEL (202) 586-5767
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This transmittal consists of 5 pages, (excluding cover sheet).

Remarks:

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reliability, and support of the assumptions of zero recombination are especially critical. The Panel's site visits have identified experimental uncertainties, e.g., nonlinearities of the calibration in power output vs. temperature, time dependence of calibration, and doubtful accuracy of data acquisition relative to the magnitude of the effects asserted. Even in laboratories that report excess heat, this effect, under apparently identical conditions, is often not reproducible. In none of our visits to the different sites did we see an operating cell that was actually producing excess heat. So far, we have seen no experimental results that are sufficiently free of ambiguities and calibration problems to make us confident that the steady production of excess heat has been observed. However, there are reports of sporadic temperature "excursions" or "bursts" that apparently represent power outputs significantly larger than the input power. These events cannot be attributed to problems with accuracy or calibration alone and are presently not understood. In general, the calorimetry to date does not persuasively demonstrate the production of excess heat, but the bursts will require evaluation in the Panel's final report.

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2. A shortcoming of most experiments reporting excess heat is that they are not accompanied in the same cell by simultaneous monitoring for equivalent fusion products. If the excess heat is to be attributed to fusion, such a claim should be supported by measurements of fusion products at commensurate levels.

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Obtain drafts of report, etc

R L Garwin

DRAFT

INTERIM REPORT OF THE COLD FUSION PANEL TO THE ENERGY RESEARCH ADVISORY BOARD

CAUTION: THIS IS NOT AN ERAB REPORT

REVISION 1 7/11/89

As a result of the startling announcement on March 23, 1989 by University of Utah scientists claiming the attainment of cold fusion, the Secretary of Energy requested (see Appendix A) that the Energy Research Advisory Board (ERAB) convene a panel (see Appendix B) to assess the possibility of cold fusion. The panel meetings and schedule of laboratory visits are summarized in Appendix C.

Since the above announcement, many laboratories worldwide have initiated research in cold fusion. In the United States, a major effort has been undertaken to search for cold fusion by a large number of highly qualified research groups at Industrial, University, and National Laboratories. Unfortunately, at the present time, the reports from different laboratories are quite divergent. Some laboratories claim excess power production attributed to cold fusion, usually for intermittent periods and for various periods of time but with no supporting evidence for the production of commensurate quantities of fusion products. Other laboratories claim no excess power production and no high levels of fusion products. Some laboratories attribute the discrepancies to inaccuracies in measurements, others to intrinsic non-reproductibility of a new and not understood process. Tritium levels above normal have been observed in some cells following electrolysis, neutrons near background have been claimed in some D_2O

electrolysis experiments but at levels 10^{13} below the amounts commensurate with the experiments claiming excess power.

Although the Panel's task is not yet complete, the experiments reported to date do not present convincing evidence that significant sources of energy will result from this phenomena attributed to cold fusion. Hence, no special programs to establish cold fusion research centers or to support new efforts to find cold fusion are justified at the present time.

However, there remain unresolved issues stemming from reported cold fusion efforts. Resolution of these issues, some of which are relevant to the mission of DOE, should be handled within current programs by normal mechanisms for selecting projects for support.

The justification for the foregoing conclusions is presented below, where the reports of excess heat and fusion products are assessed in separate sections. Preliminary recommendations are summarized in the final section.

CALORIMETRY AND EXCESS HEAT

The claim for electrochemically charged palladium cells as a potential energy source rests on reports of "excess heat" (or, more precisely, excess power) that cannot be accounted for in the thermal balance normally applied to water electrolysis. One of the questions the Panel addressed in site visits is whether the power levels themselves are being accurately measured or whether the reactions being considered in these cells are, in fact, satisfying the

chemical assumptions made. These heat measurements have been done with calorimetry varied as to technique and levels of precision and accuracy. For the purposes of this report, two types of calorimetry are differentiated by whether the D_2 and O_2 gases are allowed to exit the cell completely unreacted or are catalytically recombined to D_2O as recovered heat.

In the case of open cells, where the gases are assumed to be completely nonreactive, any output power (as heat) greater than the electrical input power minus the voltage equivalent of the D_2O formation enthalpy [1.527 V x I (cell current)] is considered excess, as reported by several groups. In closed cells, with total recombination, the enthalpy of the D_2/O_2 reaction is restored to the steady state heat balance of the fully deuterium-charged electrode such that the total electrical power in and total heat power out would normally balance (as for Pt and Pd electrodes in light water). At present, no experimenters who have performed calorimetry under strict recombination conditions (closed cells) reported any excess heat. Another important point is that most of the reported measurements with open cells are actually instantaneous power measurements and the data have not conclusively demonstrated that the total amount of energy produced (as heat and chemical energy) exceeds the total electrical energy input.

Since the claimed excess heats have, in most cases, been of a magnitude significantly less than the 1.527 I factor itself, issues of calibration, reliability, and support of the assumptions of zero recombination are especially critical. The Panel's site visits have identified experimental uncertainties, e.g., nonlinearities of the calibration in power outputs vs. temperature, time dependence of calibrations, and doubtful accuracies of data

acquisition relative to the magnitude of the effects asserted. Even in laboratories that report excess heat, this effect, under apparently identical conditions, is often not reproducible. In none of our visits to the different sites did we see an operating cell that was actually producing excess heat. So far, we have seen no experimental results that are sufficiently free of ambiguities and calibration problems to make us confident that the steady production of excess heat has been observed. However, there are reports of temperature "excursions" or "bursts" that apparently represent power outputs significantly larger than the input power. These events cannot be explained by problems with accuracy or calibration alone (and require particle measurements to determine whether their origin is due to fusion or chemical processes). Other than these presently inexplicable events, the calorimetry to date indicative of excess heat is not conclusive.

PARTICLES

Since deuterium fusion necessarily produces fusion products (neutrons, protons, tritium, 3He, 4He, xrays and/or gamma rays), it is essential to establish the presence of such products in any claim of fusion. Each watt of power must be accompanied qualitatively by 10^{12} particles per second. This makes product detection, by twelve orders of magnitude, the most sensitive method to search for fusion. Results to date on product production are summarized in the following paragraphs.

Although many experiments report no neutrons, some have reported on the order of 0.1 neutron per second. If confirmed, this would be of some scientific interest (even if not cold fusion), but because this is so far below the 10^{12} neutrons per second required for 1 watt, anticipated by established fusion

reactions (about one-half of which produce neutrons), it has no current applicability as an energy source.

Numerous experiments have sought tritium production in electrochemical cells. One group of experimenters reports production in several cells of up to some 10^{10} tritium atoms per second (neglecting losses to the gas phase) for Pd cathodes and Ni anodes. The same laboratory reports neutrons produced from similar electrochemical cells, but at a rate of about one neutron per second; if the tritium were a result of deuterium fusion, neutron production should be some 10 billion times grater than reported. Additional measurements of tritium in cells reported to produce excess heat are urgently needed by independent groups.

3He and 4He (if one assumes the highly unlikely reaction d+d - 4He + lattice energy is the dominant fusion reaction) are other important fusion signatures which should be present in detectable quantities (especially within the metal of the cathodes) at fusion levels of 1 watt. None of the searches to date (including those reporting the production of heat) have reported the generation of He above the detectable level of 10^9 atoms which is one millionth the amount of helium produced in one hour be one watt of fusion.

INTERIM RECOMMENDATIONS

1. The Panel recommends that the cold fusion research efforts in the area of heat production focus primarily on confirming or disproving reports of excess heat. Emphasis should be placed on calorimetry with closed systems with total gas recombination, use of alternate calorimetric

methods, and exchange of "promising" electrodes between groups and careful estimation of systematic and random errors. Cooperative experiments are encouraged to quickly resolve some of the claims and counter claims in calorimetry. Such experiments should be pursued at a limited number of laboratories and supported on the basis of competitive proposals, at a modest level. At the present time, the panel recommends against any significant expenditures to establish cold fusion research centers or to support new efforts to find cold fusion.

2. A shortcoming of experiments reporting excess heat is that they are not accompanied in the same cell at the same time by the expected equivalent production of fusion products. If the excess heat is to be attributed to fusion, such a claim should be supported at a commensurate production of fusion products.

3. Experiments reporting production of fusion products (e.g., neutrons) at a very low level, if confirmed, are of scientific interest but cannot account for the reported excess heat and have no apparent applications to the production of useful energy. Continuation of research support of such experiments at modest levels is justified provided the proposals for such research are evaluated in comparison with other DOE research proposals. Considering the difficulty of these experiments, collaborative efforts are encouraged to maximize the detection efficiencies and minimize the background.

Possible addition to "Particles"

Possible low-level cold fusion has also been inferred in geologic processes. $^3He/^4He$ ratios are anomalously high in volatiles from deep-source volcanoes such as Hawaii, Iceland, and Yellowstone; anomalous ^3H from Hawaiian volcanoes is also suggested by fragmentary data. Although the high ^3He may be relict from early earth processes, presence of anomalous ^3H (beyond that due to bomb tests) would be definitive evidence of natural cold fusion at depth within the earth. Implications would be major for geophysical problems such as heat-flow modelling, element-distribution with depth, and composition of the core. Rigorous search for ^3H in volcanic volatiles is being initiated at several government and university labs.

Laboratory experiments attempting to detect cold fusion will aid in interpreting these findings.

R Parmentier 07/11/89

DRAFT

INTERIM REPORT OF THE COLD FUSION PANEL TO THE ENERGY RESEARCH ADVISORY BOARD

CAUTION: THIS IS NOT AN ERAB REPORT

As a result of the startling announcement on March 23, 1989 by University of Utah scientists claiming the attainment of cold fusion, the Secretary of Energy requested (see Appendix A) that the Energy Research Advisory Board (ERAB) convene a panel (see Appendix B) to assess the possibility of cold fusion. The panel meetings and schedule of laboratory visits are summarized in Appendix C.

Since the above announcement, many laboratories worldwide have initiated research in cold fusion. In the United States, a major effort has been undertaken to search for cold fusion by a large number of highly qualified research groups at Industrial, University and National Laboratories. Although the Panel's task is not yet complete, it is unlikely that significant sources of energy will result from cold fusion in the near term, if ever. Hence, no program giving significant expenditures to establish cold fusion research centers or to support new efforts to find cold fusion are justified based on any of the work reported in the open literature or made available to us during our eight weeks of study.

Despite the present lack of basis for claiming a new source of energy, there remain some interesting scientific issues stemming from the cold fusion efforts that are unresolved. Resolution of the basic science issues, some of

which are relevant to the mission of DOE, does not require a special initiative and should be dealt with in a manner consistent with selecting for support the best science projects proposed from the scientific community.

The justification for the foregoing conclusion is presented below, where the reports of excess heat and fusion particles are assessed in separate sections. Preliminary recommendations are summarized in the final section.

CALORIMETRY AND EXCESS HEAT

The claim for electrochemically charged palladium cells as a potential energy source rests on reports of "excess heat" (or, more precisely, excess power) that cannot be accounted for in the thermal balance normally applied to water electrolysis. One of the questions the Panel addressed in site visits is whether the power levels themselves are being accurately measured or whether the reactions being considered in these cells are, in fact, satisfying the chemical assumptions made. These heat measurements have been done with calorimetry varied as to technique and levels of precision and accuracy. For the purposes of this report, two types of calorimetry are differentiated by whether the D_2 and O_2 gases are allowed to exit the cell completely unreacted or are catalytically recombined to D_2O as recovered heat.

In the case of open cells, where the gases are assumed to be completely nonreactive, any output power (as heat) greater than the electrical input power minus the voltage equivalent of the D_2O formation enthalpy [1.527 V x I (cell current)] is considered excess, as reported by several groups. In closed cells, with total recombination, the enthalpy of the D_2/O_2 reaction is

processes). Other than these presently inexplicable events, the calorimetry to date indicative of excess heat is not conclusive.

PARTICLES

Since deuterium fusion necessarily produces fusion particles (neutrons, protons, tritium, 3He, 4He, xrays and/or gamma rays), it is essential to establish the presence of such particles in any claim of fusion. Each watt of power must be accompanied qualitatively by 10^{12} particles per second. This makes particle detection, by ~~twelve~~ more orders of magnitude, the most sensitive method to search for fusion. Results to date on ~~particle~~ fusion production are summarized in the following paragraphs.

Although many experiments or report no neutron, (established fusion reactions produce ~~about one-half of which produce n's),~~ about one neutron per deuteron fusion or some have reported ~~on the order of 0.1~~ neutron per second. If confirmed, this would be of some scientific interest (even if not cold fusion), but because this is so far below the 10^{12} neutrons per second required for 1 watt, ~~if~~ it has no ~~current applicability~~ relevance to the primary purpose of heat production.

Numerous experiments have sought tritium production in electrochemical cells. One group of experimenters reports production in several cells of up to some 10^{10} tritium atoms per second (neglecting losses to the gas phase) for Pd cathodes and Ni anodes. The same laboratory reports neutrons produced from similar electrochemical cells, but at a rate of about one neutron per second; if the tritium were a result of deuterium fusion, neutron production should be some 10 billion times grater than reported. Additional measurements of

restored to the steady state heat balance of the fully deuterium-charged electrode such that the total electrical power in and total heat power out would normally balance (as for Pt and Pd electrodes in light water). At present, no experimenters who have performed calorimetry under strict recombination conditions (closed cells) reported any excess heat. Another important point is that most of the reported measurements with open cells are actually instantaneous power measurements and the data have not conclusively demonstrated that the total amount of energy produced (as heat and chemical energy) exceeds the total electrical energy input.

Since the claimed excess heats have, in most cases, been of a magnitude significantly less than the 1.527 I factor itself, issues of calibration, reliability, and support of the assumptions of zero recombination are especially critical. The Panel's site visits have identified experimental uncertainties, e.g., nonlinearities of the calibration in power outputs vs. temperature, time dependence of calibrations, and doubtful accuracies of data acquisition relative to the magnitude of the effects asserted. Even in laboratories that report excess heat, this effect, under apparently identical conditions, is often not reproducible. In none of our visits to the different sites did we see an operating cell that was actually producing excess heat. So far, we have seen no experimental results that are sufficiently free of ambiguities and calibration problems to make us confident that the steady production of excess heat has been observed. However, there are reports of temperature "excursions" or "bursts" that apparently represent power outputs significantly larger than the input power. These events cannot be explained by problems with accuracy or calibration alone (~~and require~~ particle ~~measurements to determine whether their origin~~ is due to ~~fusion or~~ chemical

but where fusion product are found can be found in these particular samples, the origin of the best bursts

tritium in cells reported to produce excess heat are urgently needed by independent groups.

3He and 4He (if one assumes the highly unlikely reaction d+d = 4He + lattice energy is the dominant fusion reaction) are other important fusion signatures which should be present in detectable quantities (especially within the metal of the cathodes) at fusion levels of 1 watt. None of the searches to date (including those reporting the production of heat) have reported the generation of He above the detectable level of 10^9 atoms which is one millionth the amount of helium produced in one hour by one watt of fusion.

INTERIM RECOMMENDATIONS

1. The Panel recommends that the cold fusion research efforts in the area of heat production focus primarily on confirming or disproving reports of excess heat. Emphasis should be placed on calorimetry with closed systems with total gas recombination, use of alternate calorimetric methods, and exchange of "promising" electrodes between groups. Such experiments should be pursued at a limited number of laboratories and supported on the basis of competitive proposals, at a modest level. Cooperative experiments are encouraged to quickly resolve some of the claims and counter claims in calorimetry. Hence, the panel at the present time recommends against any significant expenditures to establish cold fusion research centers or to support new efforts to find cold fusion.

2. The major shortcoming of excess heat experiments to date is that they are not accompanied in the same cell by the expected equivalent

production of nuclear particles. Hence, the Panel recommends that all future calorimetric measurements be accompanied by careful and reliable measurements of fusion particles. All claims in the future of excess heat as due to fusion must demonstrate an equivalent production of particles.

3. Experiments reporting production of particles at a very low level, if confirmed, are of scientific interest but have no apparent implications of the production of useful energy. Continuation of research support of such experiments at modest levels is justified provided the proposals for such research are evaluated in comparison with other DOE research proposals. Since very low particle backgrounds are of great importance, collaborative experiments are encouraged to maximize the particle sensitivity.

CALORIMETRY AND EXCESS HEAT

The claim for electrochemically charged palladium cells as a potential energy source rests on reports of "excess heat" (or, more precisely, excess power) that cannot be accounted for in the thermal balance normally applied to water electrolysis. Among the issues the Panel addressed in site visits were whether the power levels themselves are being accurately measured and whether the reactions being considered in these cells are, in fact, satisfying the chemical assumptions made. These heat measurements have been done with calorimetry varied as to technique and levels of precision and accuracy. For the purposes of this report, the calorimetry is usefully differentiated as to whether the D_2 and O_2 gases are allowed to exit the cell completely unreacted or are intentionally, catalytically recombined to regenerate D_2O and recovered the corresponding heat.

In the case of open cells, where the gases are assumed to be vented without reaction, any output power (as heat) greater than the electrical input power minus the power equivalent of the D_2O formation enthalpy [1.527 V x I (cell current)] is considered excess, as reported by several groups. In closed cells with total recombination, (and with a deutrenium-charged Pd electrode) the total electrical power in and total heat power out would normally balance (as for Pt and Pd electrodes in light water). At present, no experimenters who have performed calorimetry, in closed cells, under strict recombination conditions have reported any excess heat. Another important point is that most of the reported measurements with open cells are actually power measurements and the data have not conclusively demonstrated that the total

amount of energy produced (as heat and chemical energy) exceeds the total electrical energy input.

Since the claimed excess heats have, in most cases, been of a magnitude significantly less than the 1.527 I factor itself, issues of calibration, reliability, and support of the assumptions of zero recombination are especially critical. The Panel's site visits have identified experimental uncertainties, e.g., nonlinearities of the calibration in power outputs vs. temperature, time dependence of calibrations, and doubtful accuracies of data acquisition relative to the magnitude of the effects asserted. Even in laboratories that report excess heat, this effect, under apparently identical conditions, is often not reproducible. In none of our visits to the different sites did we see an operating cell that was actually producing excess heat. So far, we have seen no experimental results that are sufficiently free of ambiguities and calibration problems to make us confident that the steady production of excess heat has been observed. However, there are reports of temperature "excursions" or "bursts" that apparently represent power outputs significantly larger than the input power. These events cannot be explained by problems with accuracy or calibration alone. Other than these presently inexplicable events, calorimetry to date does not persuasively demonstrate the production of excess heat.

FUSION PRODUCTS

Draft of paragraph on measurements of excess heat
Bard, Faulkner, Happer, Miller, Wrighton

The claim for electrochemically charged palladium cells as a potential energy source rests on reports of continuous "excess heat" (or, more precisely, excess power) that cannot be accounted for in the thermal balance normally applied to water electrolysis. This quantity is suggested by Pons and Fleischmann as "inconceivable...to anything but nuclear processes since such excess power, integrated over *sufficient times*, exhausts the chemical capacity of the system to provide such energies." The question we have addressed in site visits is whether the power levels themselves are being accurately measured or whether the reactions being considered in these cells are, in fact, satisfying the chemical assumptions made. These heat measurements have been done with calorimetry varied as to technique and levels of precision and accuracy. For the purposes of this analysis, they are better differentiated by whether the D_2 and O_2 gases are allowed to exit the cell completely unreacted or are deliberately catalytically recombined to D_2O as recovered heat.

In the case of open cells, where the gases are assumed to be completely nonreactive, any output power (as heat) greater than the electrical input power minus the voltage equivalent of the D_2O formation enthalpy (11.527 V x cell current, i) is considered excess, as claimed in work at Utah, Texas A&M, and Stanford, in particular. In closed cells, with total recombination, the enthalpy of the D_2/O_2 reaction is restored to the steady state heat balance of the fully deuterium-charged electrode such that the total electrical power in and total heat power out would normally balance (as for Pt electrodes or Pd in light water). No workers who have performed calorimetry under strict recombination conditions (a Pt-metal catalyst in the gas space of a closed cell) have reported any excess heat. Such negative results (e.g., Pt and Pd electrode power agreement to ±0.3% at the U. of British Columbia) are always subject to the criticism that the correct experimental conditions have not been achieved. A final important point is that most of the reported measurements are actually instantaneous power measurements and the data have not demonstrated that the total amount of energy produced (as heat and chemical energy) exceeds the total electrical energy input.

Since the claimed excess heats have, in most cases, been of a magnitude significantly less than the 1.527i factor itself, issues of calibration, reliability, and support of the assumptions of zero recombination are especially critical. Our site visits have uncovered experimental problems, e.g., non-linearities of calibration in power outputs vs. temperature, time dependence of calibrations, and doubtful accuracies of data acquisition relative to magnitude of the effects asserted. Even in laboratories that report excess heat, the reproducibility of this effect under apparently identical conditions has often been poor. In none of our visits to the different sites did we see an operating cell that was actually producing excess heat. So far, we have seen no experimental results that are sufficiently free of ambiguities and calibration problems to make us confident that the steady

production of excess heat has been observed. However, there are reports of temperature "excursions" or "bursts" that apparently represent power outputs significantly larger than the input power. These events cannot be explained by problems with accuracy or calibration. Other than these presently inexplicable events, the calorimetry to date indicative of excess heat is not yet persuasive. Emphasis on calorimetry with total recombination for those groups now using open cells, use of alternative calorimetric methods, and exchange of "promising" electrodes between groups should be promoted to resolve these questions.

ARGONNE NATIONAL LABORATORY

Date: 6-28-89

Physics Division
9700 S. Cass Avenue
Argonne, IL 60439-4843

Facsimile Number (312) 972-3903
Verification (312) 972-4004

FACSIMILE TRANSMISSION COVER PAGE

Message Sent To: R.L. ~~Dick~~ GARWIN

Message Sent From: JOHN SCHIFFER

FAX Telephone Number: (619) 455-3943

Transmission consists of 2 pages (cover page plus text).

Draft paragraphs regarding "particles."

Fusion would necessarily produce fusion PRODUCTS (neutrons, protons, tritium, He-3, He-4, and/or gamma rays)-- about 10**12 particles per second for 1 watt of fusion heat. W (cap)

Although many experiments report NO neutrons, some have reported on the order of 1 neutron per second. If confirmed, this would be of some scientific interest (even if not cold fusion), but ~~because so~~ far below the 10**12 neutrons per second required for 1 watt, it is of no current practical interest. and thus

Numerous experiments have sought tritium production in electrochemical cells. No experiments with Pt anodes and Pd cathodes report excess tritium beyond the level expected from electrochemical enrichment of the tritium in the original heavy water. One group of experiments reports ~~the tritium~~ production of some 10**12 tritium (insert) atoms per second, only for Pd cathodes with Ni anodes. The same laboratory reports neutrons produced from similar electrochemical cells, but at a rate of about one per second; if the tritium is indeed a result of deuterium fusion, neutron production should be about 10 billion times greater than ~~the rate~~ reported. These experiments need to be verified by independent investigators.

He-3 and He-4 are other important fusion signatures which should be present in detectable quantities (especially within the metal of the cathodes) at fusion levels of 1 watt. None of the searches to date (including those claiming the production of heat) show the generation of He above the ~~(background)~~ level of 10**9 atoms. ~~which would correspond to the generation of 1 watt of heat for a mere millionth of an hour.~~ (insert) detectable

Changes to draft telephoned in by John Schiffer 6-28-89

at 10 MeV/He = 1.6×10^{-5} ergs

$= 1.6 \times 10^{-12}$ joule

One watt is ~ 6×10^{11} He/sec

10^9 atoms is 1/600 of a sec. ~ $\frac{1}{2 \times 10^6}$ hours

PRBIT SCRIPT Q1 dated 89/06/29 08:21:55 ..... Page 1

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  Filename Filetype Fm Format  Records K-bytes  Date     Time   Label
  -------- -------- -- - ------ ------ ------ -------- -------- ------
  PARTS01  FAX      X1 V     62     56      4 89/06/26 08:41:31 RLG2.A
R; T=0.01/0.01 08:19:57
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06/29/89

RLG:
Is there any need to keep PARTS01 FAX, CLEAN, and SCRIPT files online or hardcopy?
(See attached.)

Draft as of 06/26/89 0510 PDT:

Following our meeting in Washington of 06/22/89, Bigeleisen, Hoffman, Koonin, Schiffer, and Garwin worked out first-draft wording. I am distributing this to that group (and to the staff) for further comment. It now takes into account a BITNET suggestion from Jake. I will take care of sending this section of the report to John Huizenga by 06/30/89, as it evolves.

FAX (716) 473-6889

Draft paragraphs regarding "particles."

Fusion would necessarily produce fusion PRODUCTS (neutrons, protons, tritium, He-3, He-4, and/or gamma rays)-- about 10**12 particles per second for 1 watt of fusion heat.

Although many experiments report NO neutrons, some have reported on the order of 1 neutron per second. If confirmed, this would be of some scientific interest (even if not cold fusion), but because so far below the 10**12 neutrons per second required for 1 watt, of no current practical interest.

Numerous experiments have sought tritium production in electrochemical cells. No experiments with Pt anodes and Pd cathodes report excess tritium beyond the level expected from electrochemical enrichment of the tritium in the original heavy water. One group of experiments reports tritium production of some 10**12 tritium per second only for Pd cathodes with Ni anodes. The same laboratory reports neutrons produced from similar electrochemical cells, but at a rate of about one per second; if the tritium is indeed a result of deuterium fusion, neutron production should be about 10 billion times greater than the rate reported. These experiments need to be verified by independent investigators.

He-3 and He-4 are other important fusion signatures which should be present in detectable quantities (especially within the metal of the cathodes) at fusion levels of 1 watt. None of the searches to date (including those claiming the production of heat) show the generation of He above the background level of 10**9 atoms which would correspond to the generation of 1 watt of heat for a mere millionth of an hour.

Please send suggested changes to Dick Garwin on BITNET as RLG2 AT YKTVMV or via FAX to R.L. Garwin at (619) 455-3943

CC: David Goodwin and T.G. Finn via FAX to (301) 353-5079.

To: Richard L. Garwin, MITRE, Blgd. 29
(619) 459-9701 ext. 5465

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

June 27, 1989
(Via FAX to (202) 586-3119)

Dr. William L. Woodard
Cold Fusion Panel Secretary
Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, DC 20585

Dear Dr. Woodard:

This afternoon I did make contact both with Magkubrie(sp?) and Robert Huggins. Magkubrie is very willing to meet with us on 07/06/89, and he has set time aside. However, he says that his work is being supported by EPRI, and that Tom Passell at EPRI is his contact. If Tom Passel agreed, then Magkubrie would be very pleased to receive us.

I reached Magkubrie at SRI (415) 859-3868 and Tom Passell at EPRI (415) 855-2070. Passell says that he would be happy to have Magkubrie's work reviewed by our panel, but he does not want it spread all over the newspapers. I assured him that our job was to look into this for the Secretary of Energy, and that we would certainly attempt to follow his wishes and not to discuss this matter with the Press. That pleased Passell, and I think those should be the ground rules under which we visit SRI.

Monday evening I spoke with Huggins. His telephone number at Stanford is (415) 725-2608, and at home (415) 858-1565. I wanted to understand the status of the analysis of Huggins' Pd cathodes for He-4. Huggins said that it was not a very useful thing to look for He-4, and that some people were reporting tritium, which was much more likely and significant. However, Huggins is doing nothing in analyzing for tritium, He-3 or He-4. He is "observing various heat phenomena."

Huggins was surprised that I was among the group visiting him 07/06/89, but I explained that you just had not gotten the word at the time you sent out the notice of our visit.

Accordingly, since we have a green light to visit Magkubrie, could you please schedule that? I think that I will not change my plans or airplane reservations, still anticipating arriving San Jose airport at 0813 on AA 1004 and leaving

from San Jose airport at 1450 on AA 1463. I suppose if the visit takes longer than I expect, I could delay my departure. However, I don't think that I need more than an hour with Magkubrie, and probably not more than a couple of hours with Huggins.

Could you also please call Tom Passell to let him know who will be visiting Huggins and Magkubrie. I read him the list of members of the Cold Nuclear Fusion Panel, but I did not have at hand your note indicating who all were going to Stanford.

Just call Passell at EPRI and either leave word on his answering machine or talk to him personally. If you follow instructions and talk to his secretary, I am sure that they have a FAX to which you can send the list.

As you know, I am in La Jolla these days, where I can be reached by FAX directly at (619) 455-3943-- together with Will Happer, Dave Nelson, and (sometimes) Steve Koonin.

Very best regards.

Sincerely yours,

Richard L. Garwin
Forwarded in his absence

cc:
R.L. Garwin, La Jolla.

RLG:jah:178%WLW:062789.WLW

Date: Fri, 23 Jun 89 15:14 EDT
From: <JBIGELEI@SBCCMAIL>
Subject: Particle Report - Cold Fusion
To: rlg2@yktvmv
X-Original-To: rlg2@yktvmv.bitnet

State University of New York at Stony Brook
Stony Brook, NY 11794-3400

Jacob Bigeleisen
Professor
Chemistry
516-632-7905
23-Jun-1989 03:04pm EDT

FROM: JBIGELEISEN

TO:	Remote Addressee	(_RLG2@YKTVMV.BITNET)
TO:	Remote Addressee	(_KOONIN@CALTECH.BITNET)
TO:	Remote Addressee	(_HOFFMAN@LBL.GOV)
TO:	Remote Addressee	(_SCHIFFER@ANLPHY.BITNET)

SUBJECT: Particle Report - Cold Fusion

I believe there is a significant omission in the first draft of the particle report prepared on Thursday, 22 June. Kevin Wolf has measured neutron production in the Bockris type cells - Ni anode, Pd cathode. His neutron production rate, corrected for efficiency is approximately 1 neutron/sec. This is 10 orders of magnitude lower than the tritium rates reported by the Bockris group of which Wolf is one of the coauthors (name 2 on the manuscript and before Bockris). I suggest something approaching the following be added to the report immediately after the presentation of the results of the tritium measurements and before the recommendation that independent confirmatory experiments need to be made.

"Neutrons are reported from the same laboratory to be produced in similar electrochemical cells at a rate 10 exp -10 that of the tritium. If the tritium and neutrons are produced by cold fusion, their production rates should be about equal to one another".

Jacob Bigeleisen

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.** .un 2
.sz;.sp;.mz;.fo left ;.KP ON
1521  JTML;.BR
RSA1
.KP OFF;.cb
.cm message begins here:
You received a short FAX dated 06/22/89 from J. O'M. Bockris to
Norman Ramnsey which states:

"I am glad to tell you that David Worledge of EPRI called
me this morning and told me that the Cold Fusion work
at Los Alamos has led to the confirmation of tritium
production from D2O (100xbackground); and the confirmation
of the production of excess heat.

"I have no need to comment on this very important news."
.** .un 2
Following our meeting in Washington of 06/22/89,
Bigeleisen, Hoffman, Koonin, Schiffer, and Garwin
worked out the following first-draft wording.
I am distributing this to that group
(and to the staff) for further comment.
I will take care of sending it on to John Huizenga
by 06/30/89, as it evolves.
.H4 Draft paragraphs regarding "particles."
Fusion would necessarily produce fusion
.us products
(neutrons, protons, tritium, He-3, He-4, and/or gamma rays)--
about 10**12 particles per second for 1^watt of fusion heat.
.sk
Although many experiments report
.us no
neutrons, some have reported on the order of 1^neutron per
second.
If confirmed, this would be of some scientific interest
(even if not cold fusion),
but because so far below the 10**12 neutrons per second
required for 1^watt, of no current practical interest.
.sk
Numerous experiments have sought tritium production in
electrochemical cells.
No experiments with Pt anodes and Pd cathodes report
excess tritium beyond the level expected from
electrochemical enrichment of the tritium in the
original heavy water.
One group of experiments reports tritium production
of some 10**12 tritium per second only for Pd cathodes
with Ni anodes.
These experiments need to be verified by independent
investigators.
.sk
He-3 and He-4 are other important fusion signatures which
should be present in detectable quantities
(especially within the metal of the cathodes)
at fusion levels of 1^watt.
None of the searches to date
(including those claiming the production of heat)
show the generation of He above the background level
of 10**9 atoms which would correspond to the generation
of 1^watt of heat for a mere millionth of an hour.
  **RLG will perfect when he sees this.**
.FO OFF;.SK 4
.** .un 2
```

D Koonin

Draft as of 06/24/89 0945 PDT:

Following our meeting in Washington of 06/22/89, Bigeleisen, Hoffman, Koonin, Schiffer, and Garwin worked out the following first-draft wording. I am distributing this to that group (and to the staff) for further comment. I will take care of sending it on to John Huizenga by 06/30/89, as it evolves.

Draft paragraphs regarding "particles":

Fusion would necessarily produce fusion ~~.us products~~ products (neutrons, protons, tritium, He-3, He-4, and/or gamma rays)--about 10**12 particles per second for 1^watt of fusion heat.

Although many experiments report NO neutrons, some have reported on the order of 1^neutron per second. If confirmed, this would be of some scientific interest (even if not cold fusion), but because so far below the 10**12 neutrons per second required for 1^watt, of no current practical interest.

Numerous experiments have sought tritium production in electrochemical cells. No experiments with Pt anodes and Pd cathodes report excess tritium beyond the level expected from electrochemical enrichment of the tritium in the original heavy water. One group of experiments reports tritium production of some 10**12 tritium per second only for Pd cathodes with Ni anodes. These experiments need to be verified by independent investigators. He-3 and He-4 are other important fusion signatures which should be present in detectable quantities (especially within the metal of the cathodes) at fusion levels of 1^watt. None of the searches to date (including those claiming the production of heat) show the generation of He above the background level of 10**9 atoms which would correspond to the generation of 1^watt of heat for a mere millionth of an hour.

** Please send suggested changes to Dick Garwin on bitnet as RLG2@YKTVMV or via FAX to (619) 455-3943 **

D1

To: FILE

Date: June 23, 1989 ← (as of 06/24/89 0900 PDT)

From: R.L. Garwin x2555 26-234 Yorktown Heights, NY
IBM Fellow and Science Advisor to the Director of Research.
VNET: RLG2 at YKTVMV;
RSA1 at YKTVMV (JoAnn T. McLoughlin, Secretary)

Sub.: Draft paragraph for ERAB Cold Fusion Panel letter report.

Following our meeting in Washington of 06/22/89, Bigeleisen, Hoffman, Koonin, Schiffer, and Garwin worked out the following first-draft wording. I am distributing this to that group (and to the staff) for further comment. I will take care of sending it on to John Huizenga by 06/30/89, as it evolves.

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Although many experiments report no neutrons, some have reported on the order of 1 neutron per second. If confirmed, this would be of some scientific interest (even if not cold fusion), but because so far below the 10**12 neutrons per second required for 1 watt, of no current practical interest.

Numerous experiments have sought tritium production in electrochemical cells. No experiments with Pt anodes and Pd cathodes report excess tritium beyond the level expected from electrochemical enrichment of the tritium in the original heavy water. One group of experiments reports tritium production of some 10**12 tritium per second only for Pd cathodes with Ni anodes. These experiments need to be verified by independent investigators.

He-3 and He-4 are other important fusion signatures which should be present in detectable quantities (especially within the metal of the cathodes) at fusion levels of 1 watt. None of the searches to date (including those claiming the production of heat) show the generation of He above the background level of 10**9 atoms which would correspond to the generation of 1 watt of heat for a mere millionth of an hour.

RLG will perfect when he sees this.

R.L. Garwin

Encl:

cc:

RLG:jtml:174%FILE:062389FILE

(1,1)

If fusion would take place, necessarily a corollary is the production of fusion products (neutrons, protons, tritium, ^3He, ^4He and gamma rays).

For 1 Watt of fusion power, about 10^{12} particles per second must be produced.

Although there are many negative experiments, some have reported neutrons at levels of some 10^{-6} neutron/second. If confirmed, this would be scientifically of some significance interesting (even if not though unlikely to be cold fusion), but because so very far below the 10^{12} neutron/second required for 1 Watt, if no neutron count

Numerous experiments have ~~also been~~ sought ~~carried out for~~ tritium production in electrolytic cells. No experiments with Pt anodes and Pd cathodes report excess ~~specific~~ tritium ~~activity~~ beyond the level expected from electrochemical enrichment. One group of experiments ~~with Ni anodes and Pd cathodes~~ report tritium production of ~ 10^{10} tritons/sec ~~only~~ for Pd cathods with Ni anods. ~~electrodes only.~~ These experiments need to be ~~very~~ verified by independent investigators.

^{3}He and ^{4}He are other ~~another~~ important fusion signatures ~~these~~ which should be present ~~in reports at~~ with level in the cathodes and not in the gas phase. None of the researches to date (including more investigating heat producing cells) have shown ~~a~~ production of ~~a~~ 10^9 or more ~~He~~ atoms of ~~He~~ ^{3}He above the background level of 10^9 atoms corresponding to ~~a~~ ~~0.01~~ 0.003 watt·sec.

28 June 1989

Professor Norman F. Ramsey
Lyman Physics Laboratory
Harvard University
Cambridge, MA 0213

Dear Professor Ramsey,

Thank you for your thoughtful letter. I, too, regret not being able to see you here at BYU but I understand the unusual circumstances.

I agree whole-heartedly that the BYU experiment on cold fusion is to be distinguished from experiments claiming excess heat.

We are pleased with results which evidently confirm the production of neutrons as deuterium is loaded into metals, at Los Alamos and the Gran Sasso laboratory, for example. I am mailing to you pre-prints of papers on these experiments which were performed jointly with our research group. The Gran Sasso paper has been accepted for publication in Il Nuovo Cimento, and the Los Alamos/BYU paper has been submitted to **Nature**. We are also endeavoring to help where possible those groups who have looked but not yet observed the cold fusion effect. In many cases, these experiments followed the Pons/Fleischmann prescription which is markedly different from ours, both in the electrolyte and in the electrodes used. The prescribed durations of the experiments also differ greatly.

Naturally, I am a bit perplexed by papers which follow the prescription of someone else's experiment, then say that our experiment doesn't work! For example, a paper on "Electrochemical Experiments in Cold Nuclear Fusion" in Phys. Rev. Letters, 19 June, 1989 by J. F. Ziegler et al., uses the electrodes and the electrolyte of the Pons/Fleischmann experiment in an nice experiment to look for charged particles, such as protons and tritons from deuteron-deuteron fusion. No charged species are detected above background, and upper limits are compared with results of Pons/Fleischmann but also with those of Jones, (The latter were incorrectly normalized in their paper, being too high by a factor of about 10).

Although this group has not taken the opportunity to contact us, we would be glad to cooperate in an effort to actually test our results. This would require that they follow a markedly different prescription in the set-up of the electrochemical cell. If they have difficulty understanding the prescription presented in our Nature paper, we would be most happy to answer their questions and even provide materials as we have done for others. (Other researchers have been able to follow our prescription.) In addition, I would strongly urge that they use a thinner foils than 25 μm of Pd plus 1.7 - 6.5 μm

of gold, since it is a distinct possibility that fusions could occur on the "charging" surface of the foil in our experiments (as discussed in our Nature article), and fusion-generated protons would clearly lose too much energy traversing the thick foils to be effectively measured in the surface-barrier detector. (I am assuming an energy threshold of about 700 KeV as is typical for similar detectors used in muon-catalyzed fusion experiments which we have performed at Los Alamos and at the Rutherford-Appleton Laboratoy. In these experiments, we detect alpha and alpha-muon ions using surface-barrier detectors.)

As I write this, I realize that I am sensitive to having our work on cold fusion, which has its roots in our extensive studies of muon-catalyzed fusion, tossed indiscriminately in with the work of Pons and Fleischmann. I request that the ERAB committee look at our research as separate and distinct from theirs. The results, conclusions, methods, and bases of the experiments differ greatly. Based on our experiments at BYU which go back to May, 1986, I believe that there is interesting physics to be learned from cold fusion research. The excess heat may be an interesting phenomenon, but there is no evidence that it stems from cold fusion (certainly not from the paper of Pons and Fleischmann, nor from the work of Paneth and Peters).

You ask whether the observed neutrons might arise due to microscopic "hot" fusion in the metal, perhaps as cracks form. This is a worthwhile hypothesis to explore. This mechanism implies that the neutrons should appear in bursts. While neutron bursts have been identified in our work at Los Alamos, we also find random emissions of neutron singles. Moreover, it is difficult to account for the production of over a hundred neutrons in a burst, as we have repeatedly seen, by <u>any</u> mechanism which I have looked at to date. There is almost certainly new physics here. There are quite possibly different mechanisms responsible for the large bursts and the random neutron emissions. I hope that we can continue to receive adequate support to study these and other intriguing aspects of this new phenomenon which we call "cold fusion."

Sincerely,

Steven E. Jones 6-28-89

Steven E. Jones
Brigham Young University

Facsimile Lead Sheet

To: William Woodard
Secretary, Cold Fusion Panel
Er-6, 3F-043, US DOE

Phone: 202-586-5767
FAX: 202-586-3119

From: Dr. Allen J. Bard
Department of Chemistry
University of Texas at Austin
Phone: 512-471-3761
FAX: 512-471-8696

Number of Pages Including Lead Sheet: 14

HERE ARE A FEW RELATIVELY OBSCURE BUT PERHAPS INTERESTING PIECES ON COLD FUSION.

AJB

observed a continuous excess of enthalpy for 48 h, we wish to conclude that this may not be occurring in our experiments.

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Correction. Abstract (1st) should read 17 lines not 16 because

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20th. June, 1989.

Professor A J Bard
Editor
Journal of the American Chemical Society
Department of Chemistry
University of Texas
Austin
Texas 78712
USA

JUN 26 1989

Dear Al,

It was a great pleasure to meet you in Hawaii and see how well you have recovered from your health problem of last year.

I enclose a copy of a meeting held at the Australian Atomic Energy Commission. The speakers represent the majority of those who have been active in the cold fusion field in Australia. None of these reported any excess heat, neutrons, gamma rays or tritium or helium. Similarly, my own research group have not detected any response. Thus, there has been a completely negative experimental finding in Australia. I would of course emphasise that given that most of us have only tried for a few weeks, that the results could not be regarded as conclusive. As far as I can ascertain, two research groups have been funded to continue their studies. This represents the CSIRO group in Melbourne under the direction of Stephen Fletcher and the Atomic Energy Commission have established a task force who are working on this topic. There may also be one or two industry or university groups continuing in the field. As soon as I get a copy of the proceedings of the conference from Sydney I will pass these on to you.

Best wishes,

Yours sincerely,

Professor A.M. Bond
Professor of Chemistry

Mrc 128.89 Literaturhinweis

Bundesdeutsche Wissenschaftler widerlegten die Kernfusion im Reagenzglas

Der Traum vom Nobelpreis ist ausgeträumt

Pons und Fleischmann haben ihre Veröffentlichung in „nature" zurückgezogen – Von Carsten Schroeder

VDI-N, Berlin, 28. 4. 89 –
Aus. Ende. Vorbei. Stanley Pons' und Martin Fleischmanns Traum von der Kernfusion im Reagenzglas ist ausgeträumt. Was sie beobachtet haben, sind ganz normale chemische Reaktionen. Einziger Trost für die beiden: Mit ihnen haben sich auch all die Wissenschaftler geirrt, die vorschnell von einer erfolgreichen Wiederholung des Utah-Experimentes berichteten.

Sie alle haben sich narren lassen, sind von dem Lockruf des möglichen Nobelpreises eingefangen worden und haben ein Medienspektakel angezettelt, das in der Wissenschaft bislang seinesgleichen sucht: Forschungsergebnisse wurden nur noch auf Pressekonferenzen vorgestellt. Bis zum heutigen Zeitpunkt gibt es allerdings nur eine wissenschaftliche Veröffentlichung zum Thema, ein Aufsatz von Pons und Fleischmann im kleinen holländischen „Journal of Elektrochemistry". Und der ist mit an Sicherheit grenzender Wahrscheinlichkeit wissenschaftlich nicht zu halten.

Es liegt in der Eigendynamik der Konfusion um die Kernfusion, daß auch die Widerlegung des angeblichen Beweises der kalten Fusion wiederum auf einer Pressekonferenz bekannt gegeben wurde. Genüßlich sezierten drei bundesdeutsche Wissenschaftler den Aufsatz von Pons und Fleischmann und widerlegten ihn Punkt für Punkt.

Hauptkritikpunkte von Prof. Gerhard Kreysa (Dechema-Institut in Frankfurt) sowie Prof. Günter Marx und Prof. Waldfried Plieth (beide Freie Universität Berlin) sind Ungenauigkeiten bei der Berechnung des angeblichen Wärmeüberschusses, ungenaue Messungen von Neutronen, Gammastrahlung und Tritium (Pons und Fleischmann bauen gerade auf diesen Zahlen ihre Argumentation auf) sowie mangelhafte Fehlerabschätzungen bei den Experimenten.

Die drei Wissenschaftler hatten den Versuch nachvollzogen und noch vor zwei Wochen die gleichen Beobachtungen wie Pons und Fleischmann gemacht. Kreysa: „Wir dachten schon, wir hätten es." Doch anders als die Forscher-Teams, die an dieser Stelle abgebrochen hatten, um die Bestätigung des Experimentes auf einer Pressekonferenz zu veröffentlichen, klopften sie ihre Ergebnisse noch einmal auf mögliche Fehler ab. Und danach blieb kein Stein mehr auf dem anderen.

Zunächst die starke Wärmeentwicklung: Sie kommt dadurch zustande, daß das Deuterium in das Innere des Palladiums diffundiert. Dann steigt es nach oben und erreicht die nicht mehr von schwerem Wasser geschützte Oberfläche der Palladium-Elektrode, die katalytisch aktiv ist. Dort kommt es dann zu einer Knallgasreaktion mit dem Wasserstoff im Palladium und dem Sauerstoff aus der Luft. Dabei entsteht neben Wasser auch eine erhebliche Wärme.

Diese sogenannte katalytische Rekombinationswärme ist im Aufsatz von Pons und Fleischmann „mit keiner Silbe erwähnt", moniert Kreysa. Vergessen haben die beiden auch die Hydridbildungswärme, die dann entsteht, wenn Palladium Deuterium aufnimmt.

Rechnet man diese beiden Faktoren ein, so kommt man bei neun der elf Experimente von Pons und Fleischmann zu dem Ergebnis, daß keine Überschußwärme entstanden ist, daß also nicht mehr Energie gewonnen wurde, als man in die Apparatur hineingesteckt hatte. „Nur zwei Versuche bleiben übrig, bei denen das anders ist", so Kreysa, „und für die würde ich gern einmal eine detaillierte Fehlerbeschreibung sehen."

Aber auch die von Pons und Fleischmann beschriebene Gammastrahlung hat eine andere Erklärung. Sie rührt von einem Wismut-Isotop (Bi-214) her, das in jedem Gebäude auftritt. Dessen Strahlung unterscheidet sich von den Gammastrahlen aus dem Wasserbad nur sehr gering. Um sie auseinanderhalten zu können, braucht man eine hochauflösende Meßapparatur, die Pons und Fleischmann nicht zur Verfügung hatten. Die Radiochemiker aus Berlin freilich besaßen dieses Gerät und stellten fest: keine Gammastrahlung, sondern Wismut-Strahlung.

Das Gedankengebäude von Pons und Fleischmann ist damit wie ein Kartenhaus eingestürzt. Die Gutachter der britischen Wissenschaftszeitschrift „nature", bei der die beiden ihren lang erwarteten Aufsatz eingereicht hatten, müssen ähnliche Bedenken wie die drei bundesdeutschen Wissenschaftler gehabt haben. Als sie die beiden Autoren baten, ihnen ein paar Fragen zu beantworten, zogen Pons und Fleischmann ihren Aufsatz zurück. Offizielle Begründung: Sie hätten keine Zeit, weil sie verschiedene Details ihrer ursprünglichen Arbeit überprüfen müßten.

THE AUSTRALIAN INSTITUTE OF NUCLEAR SCIENCE AND ENGINEERING

A COLLOQUIUM

C O L D F U S I O N - FACT OR FICTION ?

The announcement by two scientists a few weeks ago that they had sustained a fusion reaction in a test tube at room temperature has created world-wide interest. If, as is claimed, the discovery will be relatively easy to make into a usable technology for generating heat and power, the implications are enormous.

Australian scientists have been quick off the mark in trying to reproduce the results of Stanley Pons and Martin Fleischmann, the two chemists who made the announcement at a press conference at the University of Utah. However, in order to establish that true fusion has taken place, it is necessary to have access to specialised neutron and gamma-ray detection equipment. Researchers also warn that this experiment is definitely not "user friendly" - so much energy was released in the original experiment that the apparatus was vaporised! These considerations, and a ready supply of heavy water, make the Lucas Heights Research Laboratories an ideal location to carry out such experiments. Many other groups are also actively studying this phenomenon - particularly the university researchers who have been closely associated with AINSE.

AINSE is responding to the interests of its members by holding a colloquium on this topic. Speakers will be invited from all over Australia to review the known facts, discuss the chemistry and physics of the phenomenon, and speculate on the future role of Australian science in developing this discovery. The involvement of any other interested groups or individuals would be welcomed.

VENUE:	AINSE Theatre, Lucas Heights Research Laboratories, N.S.W.
DATE:	Friday, 19th May 1989
TIME:	2.00 pm - 6.00 pm
COST:	$20

Overnight accommodation and evening meals are available from:

Lucas Heights Motel	543-3437/3006
Engadine Motor Inn	520-8166
Sapphire Motor Inn	522-0444

Phone AINSE on (02)-543-3411/3376/3436 to register

Dr. R.B. Gammon
Scientific Secretary

COLLOQUIUM

"COLD FUSION - FACT OR FICTION?"

AINSE THEATRE, LUCAS HEIGHTS
Friday 19th May 1989

PROGRAM

2.00 pm	Welcoming Address Professor D.R.Miller (AINSE President) Dr P.K.Kelly (Colloquium Chairman)
2.15 pm	Dr J.Boldeman (ANSTO)
2.40 pm	Mr J.Fardy & Dr G.White (CSIRO)
3.10 pm	Prof N.Hush & Dr P.Krug (University of Sydney)
3.30 pm	Dr D.Brotherton-Ratcliffe (Flinders University)
3.45 pm	Tea-break
4.00 pm	Assoc Professor A.Oates (University of Newcastle)
4.15 pm	Dr T.Ophel (A.N.U.)
4.30 pm	Dr R.Rassool (University of Melbourne)
4.45 pm	Dr T.Quickenden (University of W.A.)
5.00 pm	Dr D.Swinkels (BHP - Utah)
5.15 pm	Open Forum
6.30 pm	Dinner (booking necessary)
7.30 pm	Informal Session (if demand warrants)

Each speaker will present an overview of the investigations carried out within the speaker's organisation and a summary of the conclusions reached to date.

The Open Forum will provide an opportunity for other active groups to present a brief statement on their involvement, and for participants to question the speakers. The future direction of research; the role of government and opportunities for industry will be explored.

Phone (02) 543-3376, 3436 or 3411 to register. Cost $20

No figures rcvd.

CALORIMETRIC STUDIES OF ELECTROLYSIS OF D2O AND H2O USING A PALLADIUM CATHODE

E.KRISHNAKUMAR, V.KRISHNAMURTHY, U.T.RAHEJA,
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RESEARCH REPORT: TFR/AMP-1-89

10 May 1989

INTRODUCTION

RESULTS

The measurements of electrolyte temperature as a function of time were made in four distinct stages. In the first stage low current densities (ca. 31.2 mA cm-2) were used for a period of 80 hours. In order to keep the power input equal for the two cells, the current through D2O was 25 mA whereas that through H2O was 24 mA. The current readings were accurate to within 1%. The power input to each cell was 0.06 W.The power input values in our experiments have an error of less than 2%. The temperature variation obtained in this stage of the experiment is shown in Fig.2. Both the D2O and H2O temperature essentially follow the variation of the ambient temperature over the 80 hour measurement period.

In the second stage of the measurements, the current density was enhanced to ca. 62.5 mA cm-2. The D2O and H2O currents were 50 mA and 52 mA, respectively, and the power input in the two cells was 0.170 W (D2O) and 0.172 W (H2O). The temperature variation from 90-117 hours is shown in Fig.3. The temperature of both electrolytes is higher than the ambient temperature, with the D2O cell temperature being consistently higher than the H2O temperature by ca.2oC. The temperature variation in both cells appears to mimic the ambient temperature fluctuations well.

In the next stage of the measurements, which lasted for nearly 30 hours, the current density used was ca. 125 mA cm-2. The D2O and H2O currents were 100 mA and 110 mA, respectively, yielding corresponding input powers of 0.43 W (D2O) and 0.42 W (H2O). The temperature variation in the two cells is depicted in Fig.4. The electrolytes in both cells reach an equilibrium temperature within a period of about 2 hours. A somewhat higher temperature (an average of 2.5oC) is seen to persist in the case of the D2O cell throughout the equilibrium region shown in Fig.4.

The final stage of the experiment, lasting 50 hours, was carried out with a current density of ca. 250 mA cm-2. The D2O and H2O currents were 200 mA and 210 mA, respectively. In addition to the initial, comparatively rapid temperature rise observed in both electrolytic cells, the two curves display a slowly diverging behavior. A temperature difference of 3oC between D2O and H2O at 155-165 hours is seen to become a temperature difference of 15oC at 190 hours. Such behavior tends to indicate a degree of conformity with results of other, recent calorimetric experiments [1-3]. However, the observed behavior (Fig.5) in our experiments can be explained without recourse to hypotheses of electrochemically-induced, cold fusion. By allowing the volumes in the electrolytic cells to drop by approximately 50% in the course of the time period between ca. 160 hours and 190 hours, the effective voltage drop across the electrodes changes; the corresponding difference in the input power to the two cells is measured to be

$$\{\text{Input power(D2O)}\}/\{\text{Input power(H2O)}\} = 1.8 \qquad (5)$$

at 190 hours (where the temperature difference is maximum). When the volumes in the two cells are restored to their original values of 20 ml each by the addition of D2O and H2O, the temperature initially falls sharply and then again reach an equilibrium at 197-200 hours. It is also of interest to note that during the period over which the input power to the D2O cell was changing (160-190 hours), the input power to the H2O cell was observed to actually decrease by 4%. Despite this, the temperature in this cell was measured to increase by 2oC.

AUSTRALIAN INSTITUTE OF NUCLEAR SCIENCE AND ENGINEERING

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[FOR INFORMATION]

In case of difficulty please telephone (02) 543-3376

To: (ALL LISTED BELOW) Fax. No.

From: Dr. R. Gammon (Scientific Secretary) Date 12th May 1989

Subject: COLD FUSION COLLOQUIUM

Further to my FAX on 28th April, the provisional program for the Cold Fusion Colloquium is as follows:-

COLLOQUIUM
"COLD FUSION - FACT OR FICTION?"
PROGRAM

2.00 p.m.	Welcoming Address Prof. D.R. Miller (AINSE President) Dr. P.K. Kelly (Colloquium Chairman)
2.15 p.m.	Dr. J. Boldeman (ANSTO)
2.45 p.m.	Dr. M. Florence (CSIRO)
3.15 p.m.	Prof. N. Hush (Univ. of Sydney)
3.30 p.m.	Dr. D. Brotherton-Ratcliffe (Flinders Univ.)
3.45 p.m.	Tea-break
4.00 p.m.	Assoc. Prof. A. Oates (Univ. of Newcastle)
4.15 p.m.	Dr. T. Ophel (A.N.U.)
4.30 p.m.	Dr. R. Rassool (Univ. of Melbourne)
4.45 p.m.	Dr. T. Quickenden (Univ. of W.A.)
5.00 p.m.	Dr. D. Swinkels (BHP - Utah)
5.15 p.m.	Open Forum
6.30 p.m.	Dinner (booking necessary)
7.30 p.m.	Informal Session (if demand warrants)

Each speaker will present an overview of the investigations carried out within the speaker's organisation and a summary of the conclusions reached to date

The Open Forum will provide an opportunity for other active groups to present a brief statement on their involvement, and for participants to question the speakers. The future direction of research; the role of government and opportunities for industry will be explored.

The time allocations are tentative at this stage. Please indicate if more or less time would be preferable for your presentation.

AINSE looks forward to your participation in this event.

non-electrolyzed water. For a description of input power variation over the measurement time period, see text.

NETUU

Following the recent experiments of Fleischmann and Pons [1] there has been considerable interest in experimental investigations aimed at exploring the possibility of initiating fusion reactions between deuterium atoms by cold, electrochemical means. In their experiments on electrolysis of D2O using a palladium cathode and a platinum anode, Fleischmann and Pons observed the generation of excess heat (over and above that ascribable to normal electrochemical heating) which was attributed to the occurrence of reactions of the type

$$D + D \longrightarrow {}^3He + n \qquad (1)$$

$$\longrightarrow {}^3T + H \qquad (2)$$

which occur via electrochemical reactions at the cathode:

$$e + D^+ \longrightarrow D_{ads} \qquad (3)$$

$$D_2O \longleftrightarrow D^+ + OD^- \qquad (4)$$

In (4), Dads represents deuterium atoms adsorbed into the Pd lattice in an hitherto-unclear fashion which results in the effective equilibrium internuclear D-D separation (re) becoming considerably smaller than the corresponding value of 7.4 nm for isolated D2 molecules. In view of the significance and profound implications of these findings, further experimental studies are clearly warranted.

We report here results of calorimetric experiments on electrolysis of D2O and H2O using Pd cathodes and Pt anodes which have been carried out continuously for a period in excess of 200 hours from 19 April - 7 May 1989. We present the fullest possible details of the procedures and techniques used in our experiments as well as the raw data obtained.

EXPERIMENTAL METHOD

Two electrolysis experiments were conducted simultaneously using 20 ml each of one molar solution of NaCl in doubly-distilled H2O and D2O. D2O purity was determined to be 99.0% by means of Fourier-Transform NMR (using a proton probe in a 500 MHz Bruker AM-500 spectrometer). High purity (99%) platinum was used as the anode material. The cathode material was palladium wire of 1 mm diameter; the total surface area used was 0.8 cm2 in each electrolytic cell. The purity of the cathode material used in our experiments was determined to be 99.9% by measuring the characteristic X-rays emitted upon bombardment by 25 keV electrons. The X-ray spectrum obtained in our measurements (see Fig.1) shows the typical Lymanal,b1,b2 lines of Pd.

The two electrolytic cells were thermally insulated (with cotton) and placed inside a Pyrex beaker. The two electrodes and a mercury thermometer were inserted through a thermally-shielded lid which not only ensured that the heat loss due to evaporative cooling was minimized but also that the inter-electrode distance was maintained constant throughout the experiments. Two highly stabilized d.c. power supplies (Kepco Models ATE15-6M and ATE75-0.7M), used in the constant-current mode, were used to supply constant power to each cell. The constant-power condition could be achieved with currents to the two cells differing by only 4%. In addition to monitoring the electrolyte temperature in the two cells, the ambient temperature was also monitored with a mercury thermometer immersed in a beaker of water.

Table 1—Generation of excess enthalpy at a Ti cathode[a]

Cathode material	c.d. mA cm^{-2}	No. of expts	Excess rate of heating, W	Excess specific heating, W cm^{-3}	Excess heating/ 100% break even[b]
Ti	33.0	3	0.133	0.044	8.2
	66.0	1	0.309	0.102	17.6
Pd	63.0	1	1.54	6.88	48.0

[a]The solution contained 1 *M* NaCl. Constant current was maintained through an Aplab current source type LVH 30/10.
[b]100% Break even means that the thermal output equals the input required to drive the equipment. The calculations were done using the total input and output power.
[c]Joule heating is obtained by separate experiment. The cell voltage for electrode reaction is 1.54 V.

Electrical power input → Electrolysis of D_2O (Electrolytic power)
Electrical power input → Joule heating (Joule power)

When the joule heating is estimated from the temperature rise, mass of the D_2O, heat capacity electrolyte, specific heat of D_2O (1.10 cal g^{-1} °C^{-1}) (ref. 9) and the heat conduction by the metals used as electrodes, and applying a correction for the rate of cooling, it is estimated that the contribution of the power input essentially equals the joule heating. Hence the electrolysis of D_2O is free! An independent experiment was conducted by heating a nichrome coil in D_2O (plus the electrolyte) with the help of a current source and adjusting the power input through a variac to maintain the solution temperature reached by the electrochemical reactor. In a typical experiment, the power required to maintain the bath temperature at 58°C was 3.24 W; the electrochemical reactor requires 3.20 W for maintaining the same temperature. Hence the electrochemical reaction or the heat produced is supported by some other reaction; and electrolysis cannot be done free as the electrons have to be added and removed at the electrodes. With the electrolysis prolonged over a period of 48 h, heat in excess of 1.19 MJ/cm^3 of the electrode volume was liberated at Pd and 0.2 MJ/cm^3 at Ti. This difference in excess enthalpy arises from the thickness of Ti (2 mm) and Pd (0.14 mm) employed in the experiments. As can be seen from Table 1, there is a substantial power gain during the experiment.

If the excess heat arises from the nuclear reaction in the electrode, it should be possible to detect the neutrons or γ-rays (by $^1_1H + ^2_1D$ reaction) produced during the fusion process. Using a BF_3 counter in front of the electrolytic cell, the rate of counting was monitored before and during the electrolysis. The rate of counting during the electrolysis was 48% higher than the background level in one experiment and in another it was only 8% higher. The liquid scintillation counter measured 18% higher than the background value suggesting both neutrons and γ-rays are coming out of the reactor. However, further experiments will have to be carried out for confirming nuclear fusion during D_2O electrolysis. During the above experiments we have observed two interesting features:

(a) the cracking noise of Ti electrode,

(b) the reddish brown colour developed by the Ti electrode. This colour can be obtained in a lighter shade by a prolonged heating of Ti metal.

With electrolysis conducted by passing 66 mA/cm^2 at the Ti cathode and the Pt anode, the estimated loss of D_2O through electrolysis alone is 0.34 mL/h; in the actual experiment the measured loss is approximately 1.0 mL/h. The excess loss arises from the excess heat evolved in the electrochemical reactor.

Since the neutron flux obtained during the electrolysis of D_2O is smaller than would be expected by at least ten orders of magnitude, other alternative mechanism can be considered for the excess heat evolved in the reactor. It may be non-emitting nuclear processes or chemical processes to sustain the heat produced. If D_2 escapes out of the electrolytic cell, it could be burnt on the surface of Pd or Ti to produce D_2O. This exothermic reaction for H_2O has an enthalpy $-\Delta H = 285.83$ kJ mol^{-1} (ref. 10). This, being a process requiring the diffusion of the electrolytically generated O_2 to the bare metal of the cathode surface, requires a diffusion of O_2 faster than H_2. Yet another possibility would be an exothermic formation of deuteride on Ti or Pd. Generally, the $-\Delta H$ for the adsorption of hydrogen decreases with increasing surface coverage on the electrode[11] and hence a prolonged coverage should result in the absence of excess enthalpy. As we have

It is intriguing that under conditions of highest current density and highest input power, even the temperature of the H2O cell rises by 2oC over a period of ca. 30 hours. This rise in temperature is of the same magnitude as the observed difference in the D2O and H2O temperatures at lower input powers and current densities (Fig.3,4).

To summarize, the results of simultaneous experiments on electrolysis of D2O and H2O, conducted over an extended period of 200 hours, provide some evidence that under conditions of constant input power, the temperature in the cell containing D2O is observed to be consistently higher (by ca. 2oC) than that in the H2O cell. We are unable to pinpoint any source of systematic error to account for such a temperature difference. On the other hand, our measurements clearly fail to provide support for other experimental findings [2,3] in which the D2O temperature rises in much more dramatic fashion.

ACKNOWLEDGEMENTS

We are grateful to many colleagues for helpful discussions and useful suggestions. In particular, it is a pleasure to acknowledge the help afforded to us by P.B.Thomas, A.K.Rajarajan, S.Modi, S.Mazumdar and A.S.Medhi.

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3. Sundry press reports (April -May 1989)

FIGURE CAPTIONS

1. X-ray spectrum of cathode material showing characteristic lines of Pd.

2. Temporal variation of temperature in cells containing D2O, H2O and non-electrolyzed water. The input power was 0.06 W.

3. Temporal variation of temperature in cells containing D2O, H2O and non-electrolyzed water. The input power in the D2O cell was 0.170 W, and that in the H2O cell was 0.172 W.

4. Temporal variation of temperature in cells containing D2O, H2O and non-electrolyzed water. Input power (D2O) = 0.43 W, input power (H2O) = 0.42 W.

5. Temporal variation of temperature in cells containing D2O, H2O and

OBSERVATION OF TRITIUM PRODUCTION DURING ELECTROLYSIS OF HEAVY WATER SAMPLES USING PALLADIUM WIRES.

N.J.C. Packham, K.L. Wolf, M.E. McLain and J.O'M. Bockris

Department of Chemistry, The Cyclotron Institute, and Department of Nuclear Engineering, Texas A&M University, College Station, Texas 77843

INTRODUCTION

A research group here has been studying an effect described by Pons and Fleischmann [1]. Research has centered around the investigation of the metallurgy of the palladium, anomalous heat production and the detection of nuclear emissions. Here, we describe the observation of tritium in seven out of eleven electrochemical cells, at levels which could not be produced by any process other than a nuclear one.

EXPERIMENTAL

The electrochemical cells used were 15 ml Pyrex centrifuge tubes, sealed with Viton rubber septa. Palladium samples supplied by the Texas Coin Exchange, 1 mm by 4 cm, and 3mm by 4 cm in dimension (99.9% purity) were prepared as shown in Table 1. All electrode connections were made using 99.9% pure nickel wire (0.5 mm). Nickel gauze anodes were used in all cases (99.9% purity). Electrode connections were fed through the rubber septum to the outside. 0.1M LiOD was prepared using 99.9% pure lithium metal from Alfa Associates, added to 99.9% pure deuterium oxide (Aldrich Chemical Co.), in an atmospheric bag containing argon. Additions to the cell (such as D_2O refilling) were performed by using disposable syringes (one use only), equipped with stainless steel needles. Gases evolved during electrolysis were

allowed to escape through a needle attached to Tygon tubing to a mineral oil bath to avoid light water contamination. All 1mm cells were at first run at 60 $mAcm^{-2}$ for 14 to 16 days, the 3mm electrodes being charged for up to 28 days. After this time, the current density was increased to 500 $mAcm^{-2}$ for periods of up to 8 hours. The cells were monitored electrochemically continuously during this time. Samples of electrolyte were withdrawn from the cell using a sterile syringe (later discarded).

Liquid Scintillation Counting (LSC) was performed using a LKB-Wallac Model 1219 Rackbeta LSC. A water soluble scintillation cocktail (Biosafe II, Research Products International Corporation) was added to 1 ml of sample. After allowing time for deexcitation of the cocktail, the samples were run in a double blind fashion. Blank samples of H_2O, D_2O, and 0.1M LiOD were also included for analysis. The efficiency of the detector for tritium was 33%. Analysis of samples was also confirmed in another instrument at this institution, and additionally by one private and three National laboratories.

RESULTS AND DISCUSSION

The activity of the samples in disintegrations per minute per ml of solution are shown in Table 1. In one of the cells (designated A7) the build up of tritium as a function of time was followed at high current density, and the results are shown in Fig. 1. The possibility of chemiluminescence of the scintillation cocktail at 0.1M LiOD was ruled out by the experimental study shown in Fig. 2.

If tritium is produced only at high current density (cf. Fig. 1), based on the observed rate of tritium production, calculation shows that in the highest activity sample (cell A3), approximately 10^{10} atoms of tritium are produced per second, neglecting losses in the gas phase (which may be

appreciable).

If tritium is produced according to the following equation:

$$^{2}H + {}^{2}H \longrightarrow {}^{3}H + p + 4.02\ MeV \qquad (1)$$

, at 10^{10} atoms per second this would be equivalent to 2 milliwatts, not detectable in the calorimeter at present in use in this laboratory. Cells A4 and B5 have also shown emissions of up to 50 neutrons per minute which will be described in detail elsewhere.

Although it has not been proved that the electrodes which produced the tritium reported here also produced the Fleischmann-Pons heat (apprroximately 10 Watts cm^{-3}), palladium electrodes prepared in a similar way did so. The ratio of total heat production to that accounted for by the tritium in solution, 5×10^{3}, is a reasonable ratio (at 1 $Ampcm^{-2}$) for the gas ($_{D}T$) retained in the solution, the rest escaping into the gas phase. The shape of figure 1 is consistent with the establishment of a quantity of DT in the electrolyte in equilibrium with DT in the escaping DT-D_2 mixture.

A problem is the sporadicity of the effect*. It is noteworthy that nickel anodes were used in the charging of electrodes which gave tritium. An alternative explanation may concern the times of charging of these electrodes. Negative results have been reported mainly for larger (>2 mm) electrodes which might not yet be ready for high current density treatment until times approaching 2 months from initiation of charging.

One may speculate that tritium only forms in an electrochemical surface reaction when dendritic growths having low radii of curvature ($<10^{-5}$cm) have

---

* But cf. Schoessow and Wethington [2] who have obtained 50,000 disintegrations per minute per ml.

formed on the surface**. Here, at the low radius of curvature tips, local electric fields of approximately 10^{10} Volts cm^{-1} may bring the D^+ ion in transfer to an energy of 10 keV*** sufficient to fuse with an adsorbed $D^{\delta+}$ on the electrode surface. The region of the electric double layer at the metal surface is known to be electron-rich [3] and thus provide the screening which may allow a D^+ having an energy in the 10 keV range to fuse with a surface D^+.

ACKNOWLEDGMENTS

We acknowledge the financial assistance of the following; The Welch Foundation, the Electric Power Research Institute and Texas A&M University. We gratefully acknowledge the assistance of the following; Ross Lemmons, Bob Sherman and Roland Jalvert of Los Alamos National Laboratory, Dave Robertson and Russ Jones of Battelle, Pacific Northwest Laboratory, Kevin Myles of Argonne National Laboratory, Dennis Corrigan of General Motors Research Laboratory, and the following from Texas A&M University: Ramesh Kainthla, Omo Velev, Jeff Wass, Lamine Kaba, Guang Lin, Marek Szklarczyk, Arpad Szucs, Babli Kapur, Maria Gamboa-Aldeco, Anuncia Gonzales-Martin, Ljiljana and Zoran Minevski, Jeng King-Tsai, Yang Bo, Lin Chen, Peter Lee, and John Shoemaker.

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2 G.J. Schoessow and J.A. Wethington, private communication, 1989.

3 W. Schmickler and D. Henderson, J. Chem. Phys., 85 (1986) 1.

** Scanning Electron Micrographs of the surface of electrodes prepared in a similar way to ours have shown dendritic growths

*** Equivalent to a temperature of approximately 10^8 ^oK

FIGURE CAPTIONS

Figure 1. The production of tritium in the electrolyte of cell A7 (see Table 1) as a function of time.

Figure 2. The effect of electrolyte concentration on chemiluminescence of the scintillation cocktail.

TABLE 1
Cell identification, electrode treatment, solution type and tritium activity of electrolyte samples.

CELL	ELECTRODE TREATMENT[a]	SOLUTION[b]	ACTIVITY (d $min^{-1}ml^{-1}$)
A1	A	1	3.8×10^4
A2	A	2	315
A3	B	1	4.9×10^6
[c]A4	B	2	1.2×10^5
A5	C	1	3.7×10^6
A6	C	2	3.3×10^4
A7	D	1	
	Before high current density		249
A7	After 2 hours at 500 $mAcm^{-2}$		5370
A7	After 6 hours at 500 $mAcm^{-2}$		5.0×10^5
A7	After 12 hours at 500 $mAcm^{-2}$		7.6×10^5
A8	D	2	339
B3 (3mm)	B	1	6.3×10^4
B5 (3mm)	C	1	195
CELL 1 (6mm)	A	1	264
D_2O			195
0.1M LiOD			225
Neutralized 0.1M LiOD			220
Neutralized 0.1M LiOD + 0.1mM NaCN			230

[a] Key for electrode surface pre-treatment: (A) No surface pre-treatment; (B) anneal 800 ^oC, 10^{-6} torr, 8 hours; (C) acid etch, 5M HCl, 15 minutes; (D) electrochemical oxide removal, 2 hours

[b] Key for solution type: (1) 0.1M LiOD; (2) 0.1M LiOD + 0.1mM NaCN

[c] Cell that has shown neutron activity up to 50 neutrons per minute.

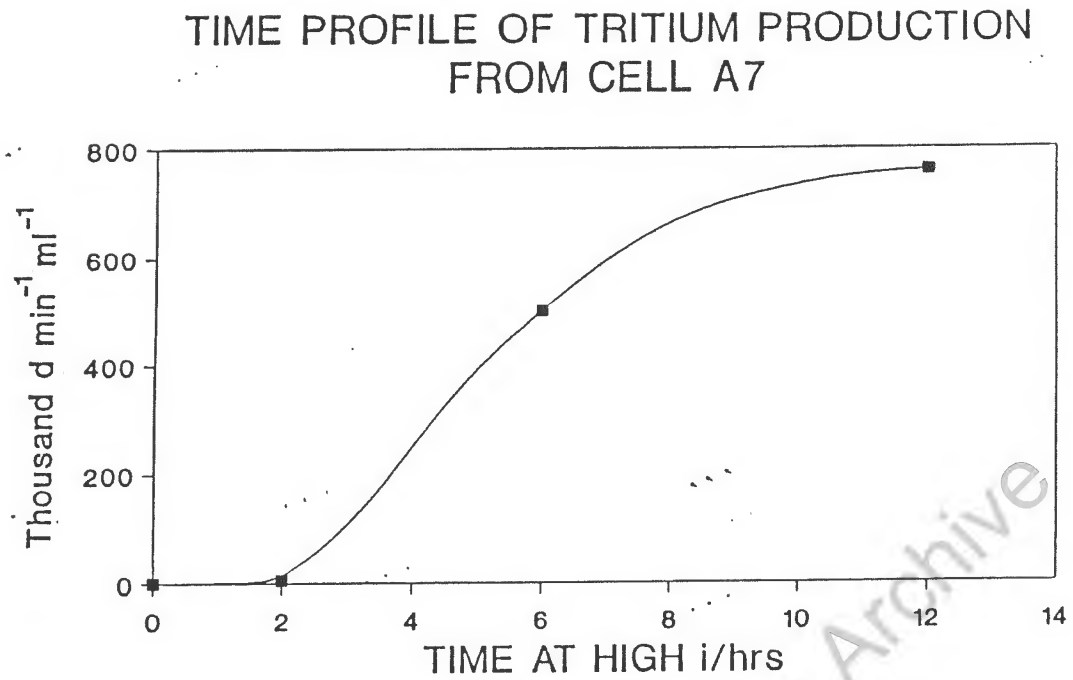

Figure 1 Packham et al Tritium Production

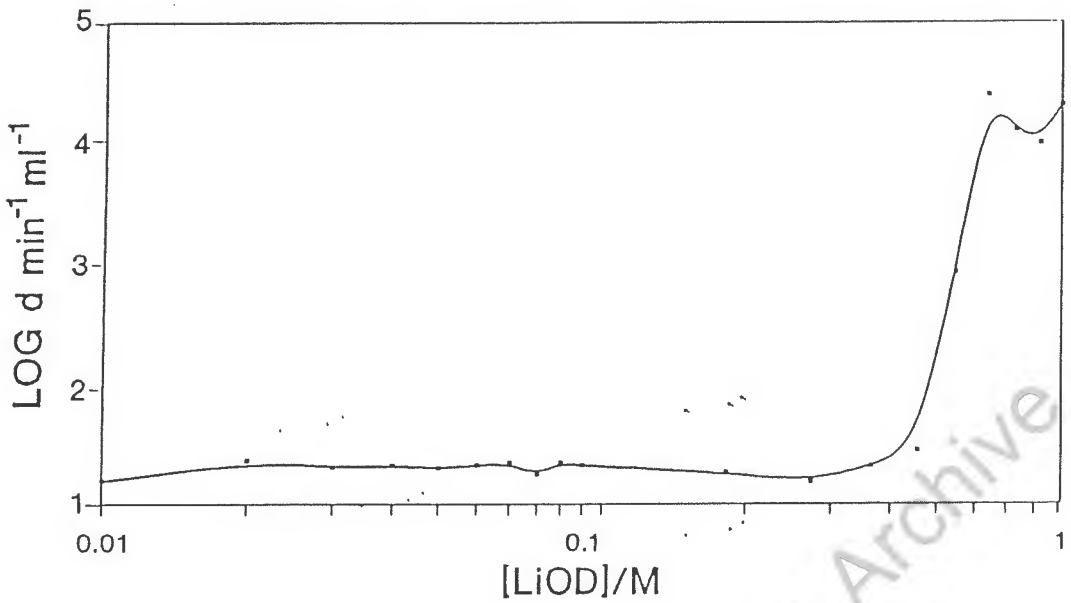

Figure 2 Packham et al. Tritium Production

TEXAS A&M UNIVERSITY
DEPARTMENT OF CHEMISTRY
COLLEGE STATION, TEXAS 77843-3255

June 21, 1989

(409) 845-2011
FAX (409) 845-4719

Dr. Jacob Bigeleisen
University of NY at Stony Brook
Stony Brook, NY 11794

Dear Dr. Bigeleisen:

I hope you have the tritium paper which I faxed you on the day you were here.

Of course, your view on our tritium measurements is more valuable than that of anyone else on the committee on cold fusion because you have the experience.

We want to regard these measurements only as facts, and we don't want to make any interpretation of them at this time. We need to know only that they are correct measurements.

In addition to the measurements which I showed you in the paper, I enclose a measurement from another electrode showing the change of tritium with time as the electrode continues its action.

It is, of course, important to consider the significance of these measurements, because errors such as one might think of - chemiluminiscence, etc. (very heavily guarded against in our measurements) - would hardly be likely to replicate the S-shaped curve which we have shown you in the first measurements, and which is to some extent replicated here.

It seems that the amount present can be fitted into a reasonable hypotheses concerning the origin of the tritium. One has to assume it is produced on the surface largely as bubbles, and produces some 5-10 watts/cc via the D + D → T + H reaction.

Then one can calculate the tritium which ought to be in the bubbles of gas as DT, and correspondingly by assuming equilibrium at high times the amount in solution.

Incidentally, I have experienced quite a few commissions of inquiry in my time, but I never found one which had its mind made up beforehand as much as this one.

I am sending you the present score as I know it on the confirmation of Fleischmann and Pons throughout the world, and I expect there's quite a lot that I don't know about.

I note the commission was out of touch even with the work going on in

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26 June 1989

Professor John O'M. Bockris,
Department of Chemistry,
Texas A&M University,
College Station, Texas 77843-3255

Dear Professor Bockris,

Let me thank you and your colleagues for your forthright presentation of your results on experiments relating to the possibility of cold fusion at our visit on 19 June 1989. I have a copy of your tritium manuscript. When I returned from our visits fof the week of 19 June, I had a message that you tried to call me on Wednesday, 21 June. I have tried several times to return your call to 409-845-4947 without success.

I thank you for your letter of 21 June sent via FAX. In that letter you mention a figure which gives an S shape curve for the rate of tritium production. There was such a curve for run A7 which was distributed with the copies of the overhead projections at the time of our visit. There was no figure attached to the FAX of 21 June. If there is an additional curve, please send it via regular mail.

The question as to whether tritium formed by cold fusion leaves the cell in the form of DT or whether most of it remains behind in the form of DTO is an interesting scientific question. We do know that all isotopic forms of $H_2(g)$ exchange slowly with liquid water. A number of years ago Stevens at Chalk River achieved a major breakthrough and developed a non-wetting platinum catalyst which has exchange times for $H_2(g)$ with liquid water of less than one second. I believe that there is a high probability that T formed at a Pd surface in a cell where the Pd is the cathode would exchange rapidly with the D_2O liquid. If, in fact, the exchange is slow, the the tritium production is 10^3 the rate found by analyzing the electrolyte. One should collect enough of the $D_2(g)$ coming out of the cell, dry it and then measure its specific activity either in an ion chamber or a proportional counter (for a description of the latter see J. Bigeleisen and E.C. Kerr, J. Chem. Phys. 39, 763 (1989).

Our Committee is much interested in your tritium measurements and are troubled by the fact that your production rate for tritium atoms, as measured by the specific activity of the electrolyte, is some 10^{10} times the neutron counting rate measured by Kevin Wolf in similar type cells. If, in fact, most of the tritium leaves in the form of DT, then the discrepancy with the neutron data becomes 10^{13}.

Sincerely,
Jacob Bigeleisen
Distinguished Professor Emeritus

xc: ERAB Panel

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LA-UR-89-1570

THE MEASUREMENT OF NEUTRON EMISSION FROM Ti PLUS D_2 GAS

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We have measured neutron emissions from cylinders of pressurized D_2 gas mixed with various forms of Ti metal chips and sponge. For some of the cases, the Ti was coated with a surface layer of Pd. The gas pressure ranged from 20 atm to 50 atm, and the Ti loadings ranged from 30 g to 200 g.

The neutrons were measured using a high efficiency (34%) cavity-type detector containing 18 ^{3}He tubes. Random neutron emissions were observed as well as time-correlated neutron bursts. The time spread in an individual burst was less than 200 μs.

The neutron emission was observed after the cylinder had cooled in liquid nitrogen temperature and was warming to room temperature. The bursts occurred about 40 minutes into the warm-up phase, and the random emission occurred for at least 12 hours after the sample reached room temperature. This cycle could only be repeated two or three times before neutron emission ceased.

The neutron emission rates were very low and the 12-hour random emission rate was 0.05-0.2 n/s. However, this yield was still 11σ above the background. The instantaneous neutron bursts were more dramatic with yields several orders of magnitude above the coincidence background rates.

Two plus two is five,

For the larger values of two.

PRIORITY LIST FOR NEUTRON (DUCK) HUNTERS

1. ARE THERE ANY DUCKS (SENSITIVITY)?

2. WHEN DO THE DUCKS COME AND GO (TIME HISTORY)?

3. HOW FAST ARE THE DUCKS (ENERGY)?*

4. DO THE DUCKS ARRIVE IN FLOCKS (PULSING)?

5. HOW MANY DUCKS ARE THERE (ABSOLUTE YIELD)?

*YIELDS ARE STILL TOO LOW TO ESTABLISH ENERGY.

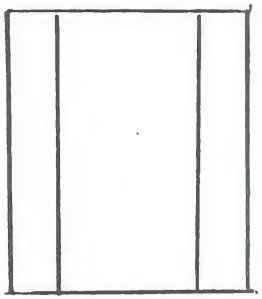
NEUTRON EVENTS

- RANDOM EMISSION
- BURSTS (Time Correlations)

ENVIRONMENTAL TESTS ON NEUTRON DETECTORS

- NO GAMMA-RAY SENSITIVITY UP TO 1 R/h
- TEMPERATURE COEFFICIENT = -0.01%/°C
- NO RF NOISE PICKUP (CRANES, MOTORS, WELDERS)
- NO MICROPHONIC NOISE
- LONG-TERM STABILITY = 0.01% (PRECISION)

SYSTEM-1



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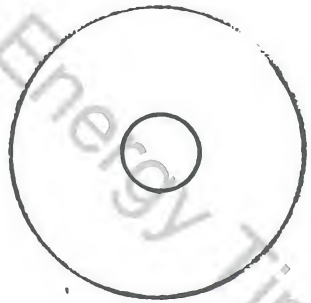
SYSTEM-2



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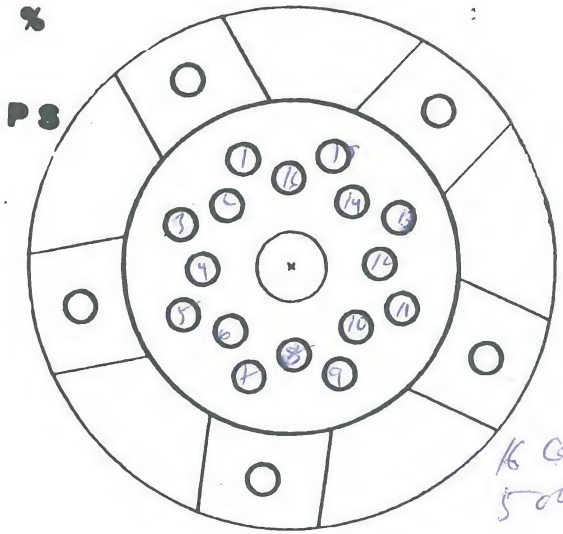
0.09 CPS

SYSTEM-3

34 %

0.24 CPS



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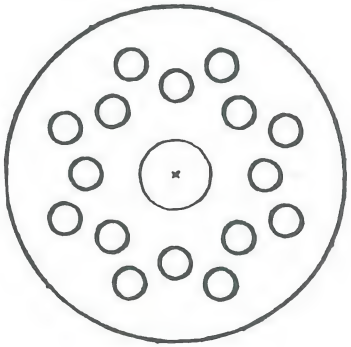
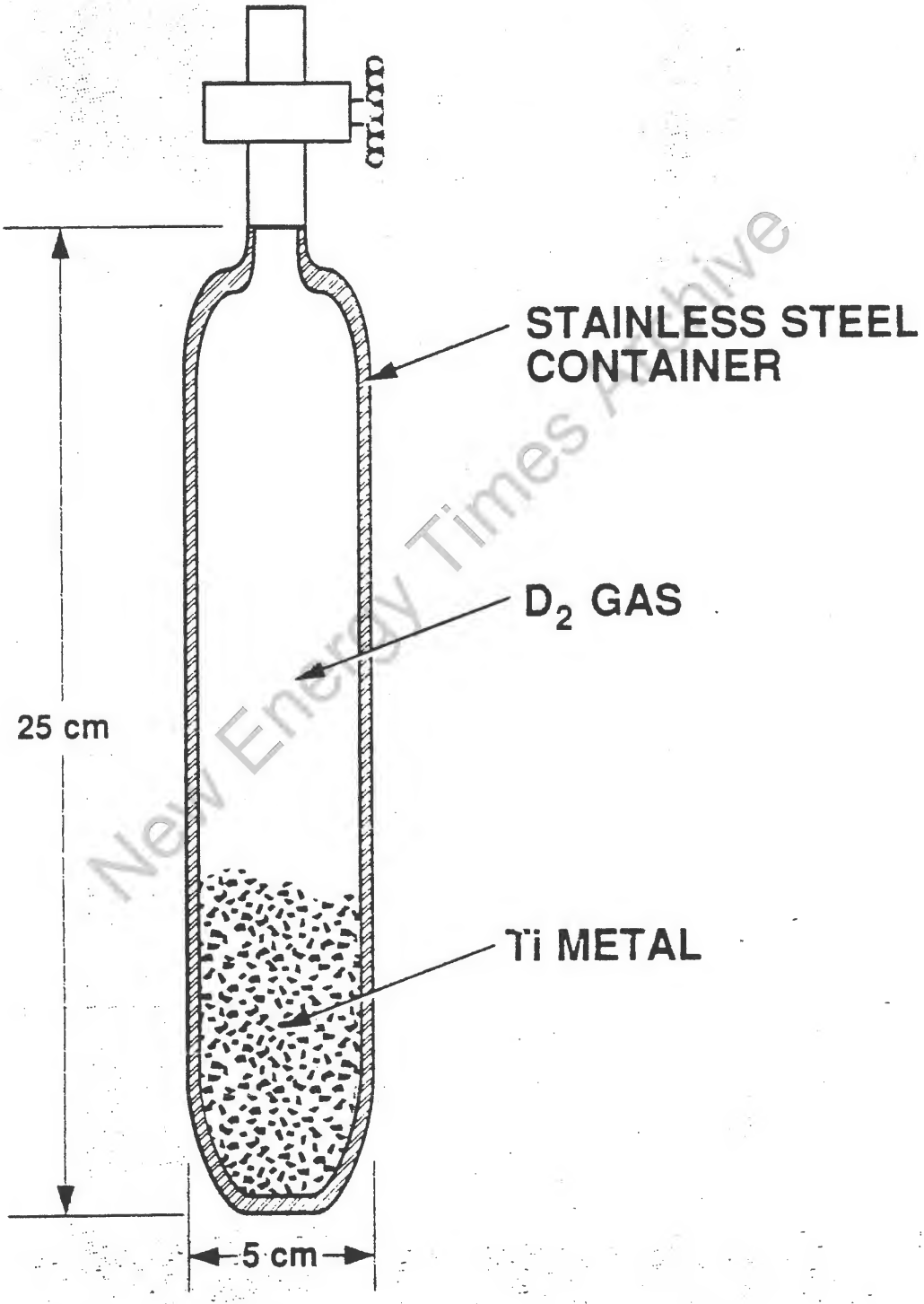
0.38 CPS

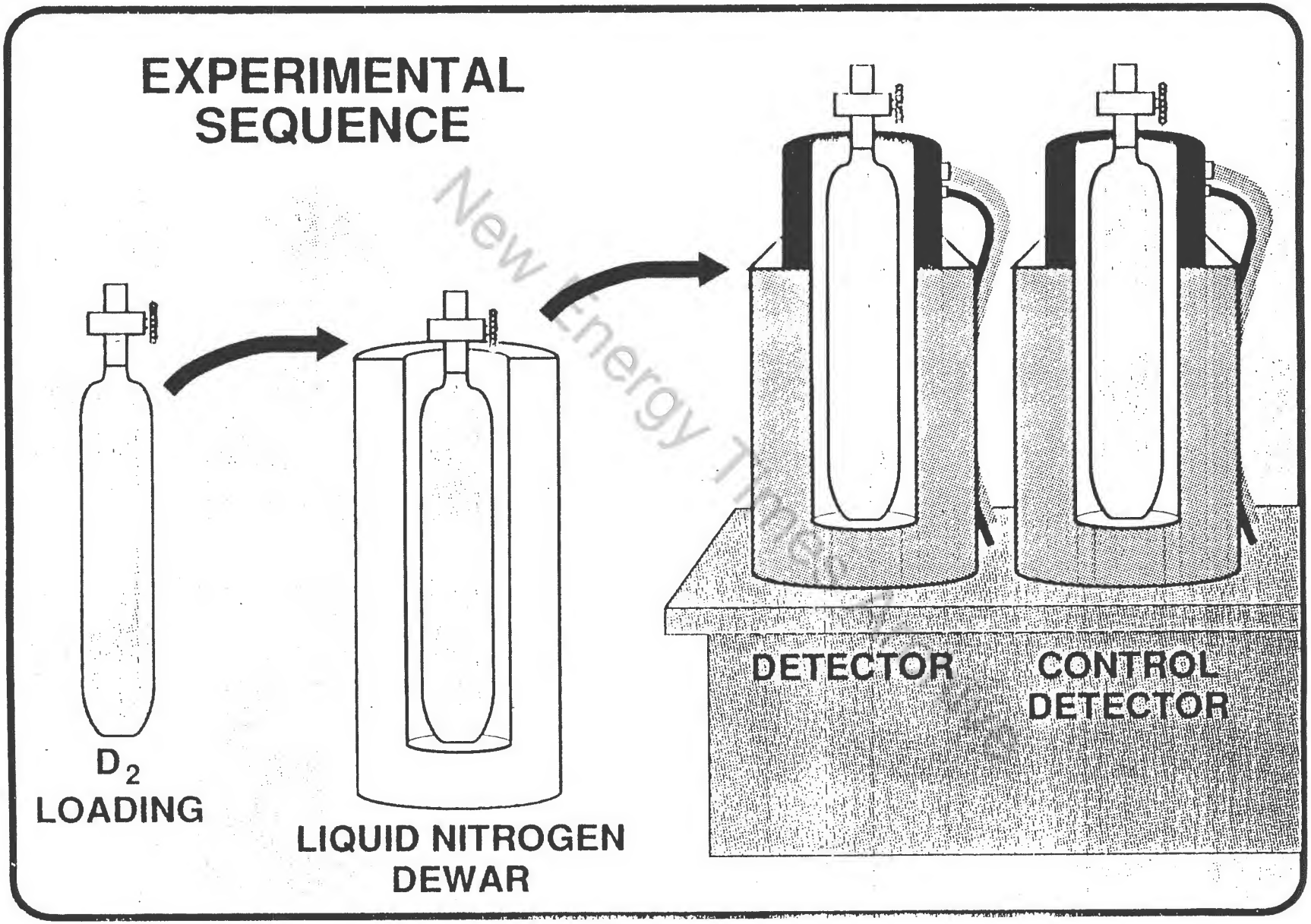


DATA CONTROLS

- TWO CONTROL COUNTERS ADJACENT TO THE PRIMARY COUNTER
- DUMMY CYLINDERS MEASURED SAME AS ACTIVE CYLINDER
- ENVIRONMENTAL CHAMBER TESTS
- THE COUNTERS HAVE BEEN OPERATED FOR MORE THAN A MONTH WITH NO SPURIOUS BURSTS

SAMPLE CYLINDER




EXPERIMENTAL SEQUENCE
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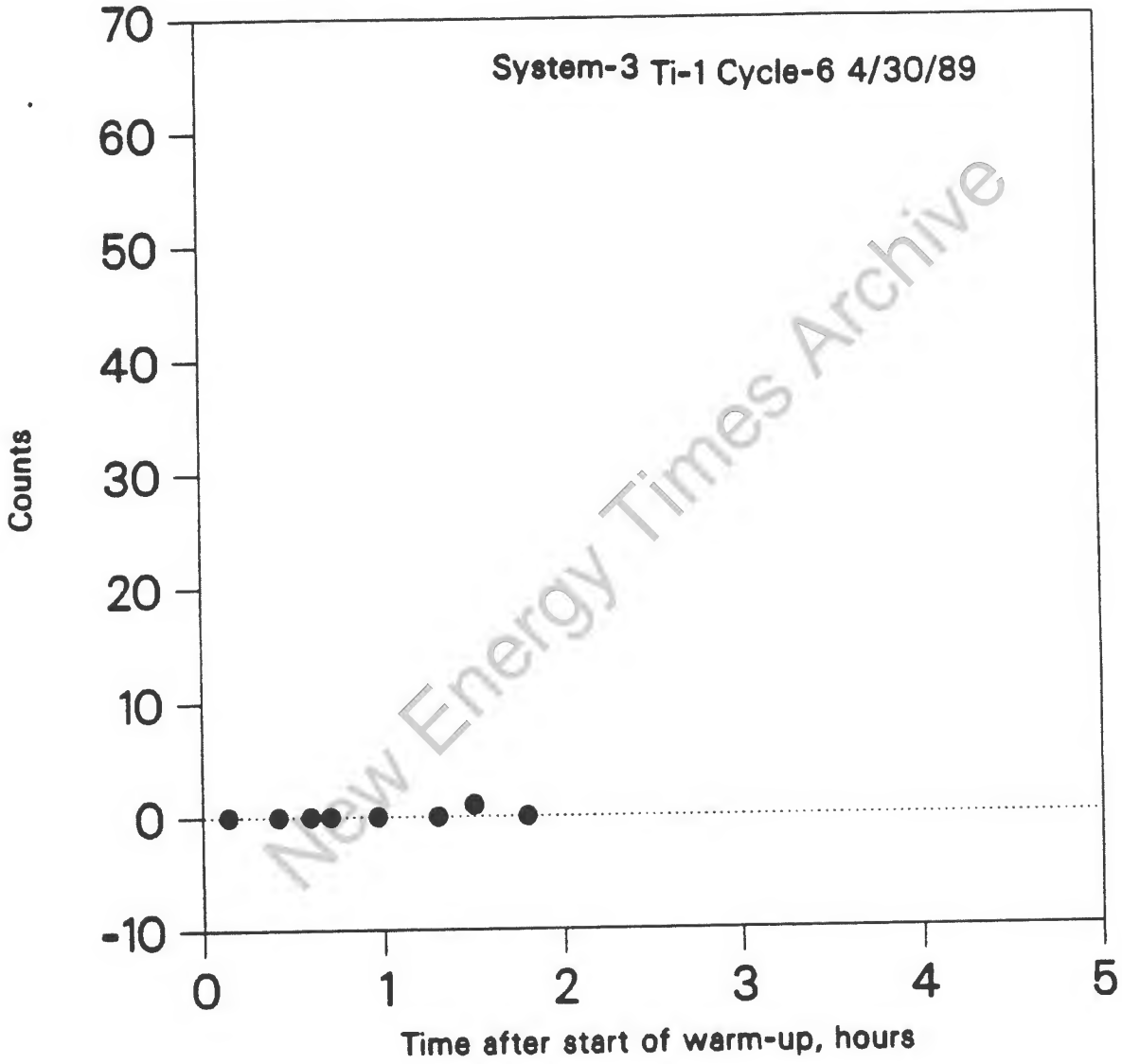
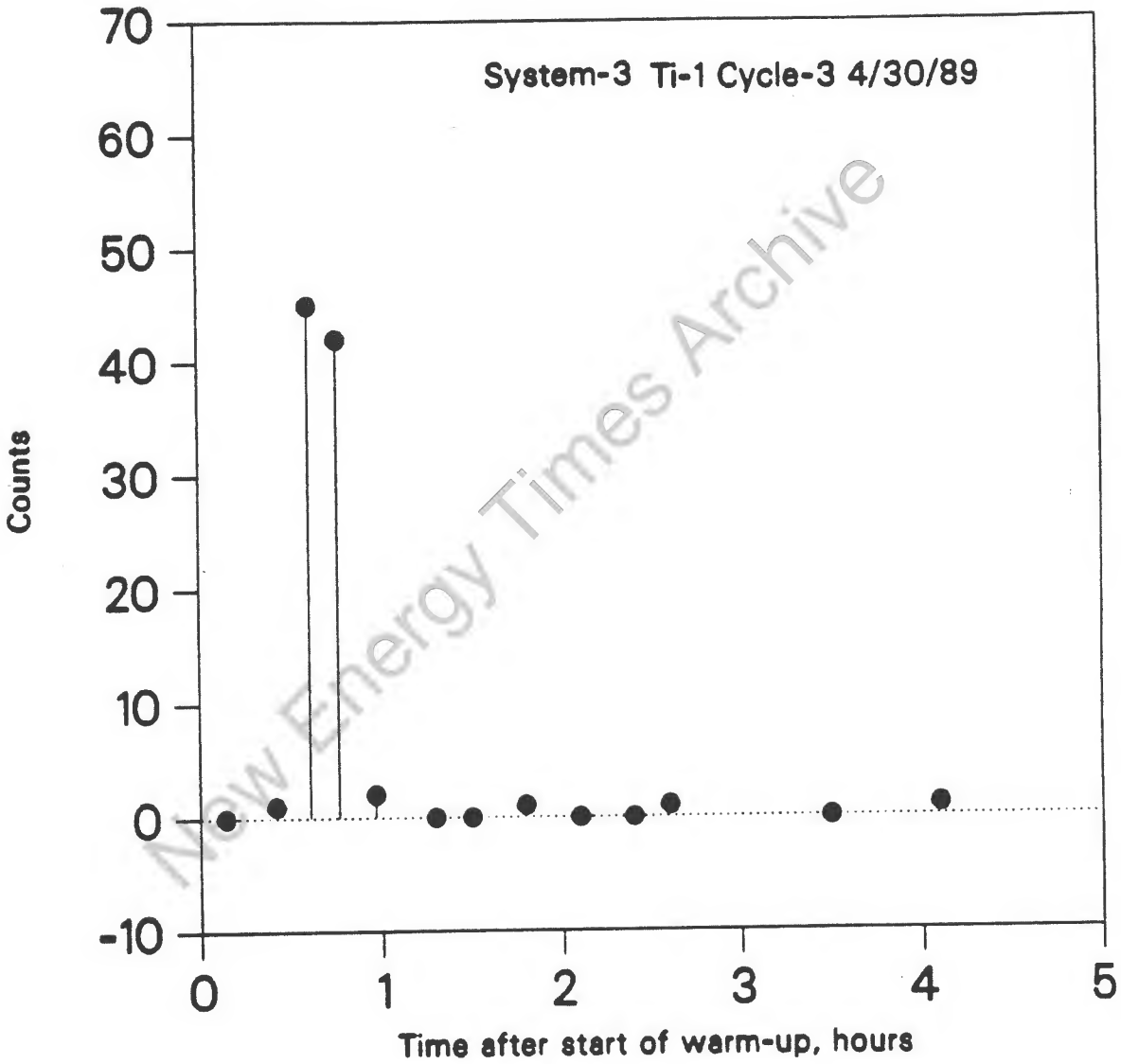
DETECTOR
CONTROL DETECTOR

NEUTRON EVENTS

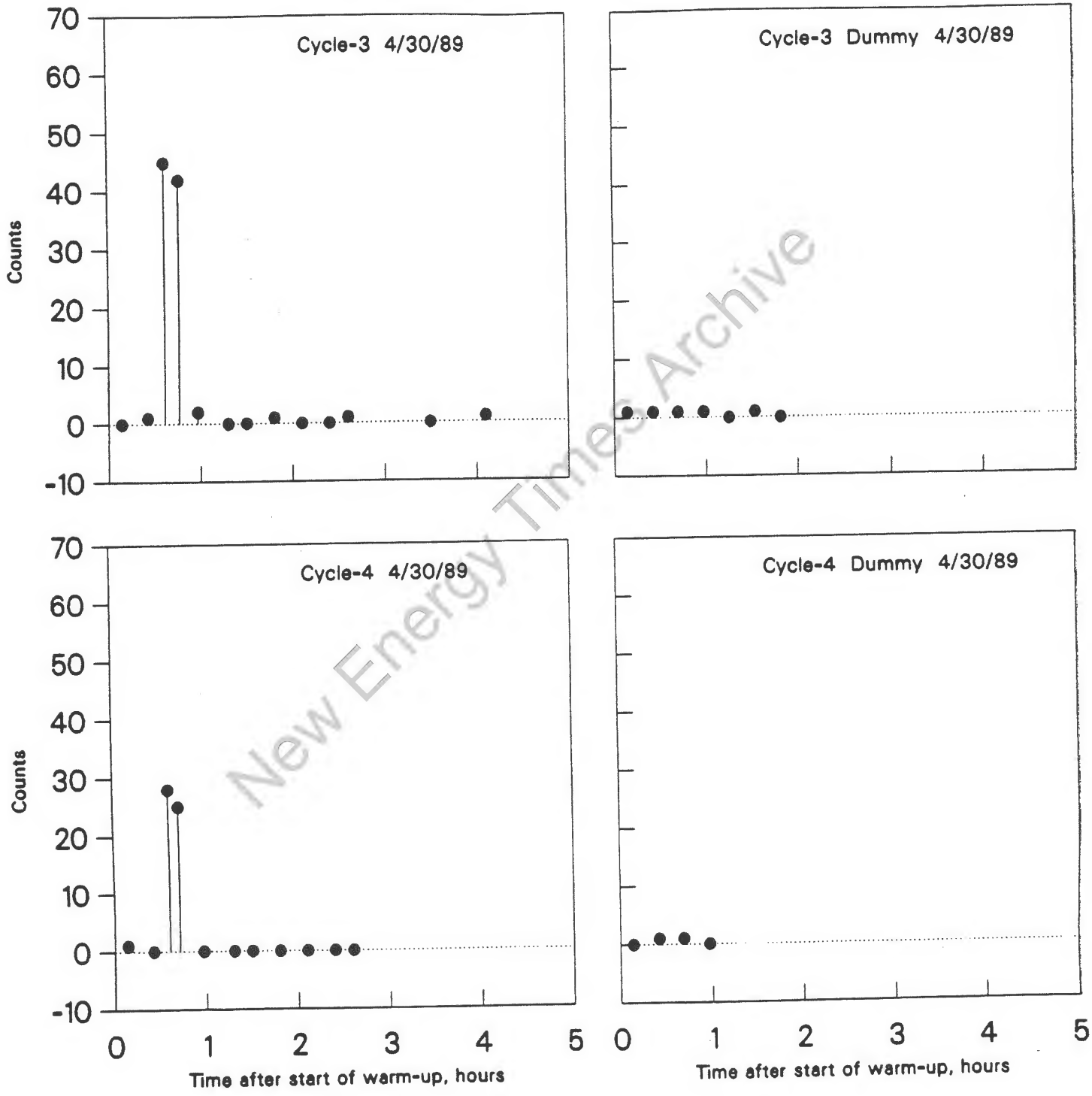
. RANDOM EMISSION

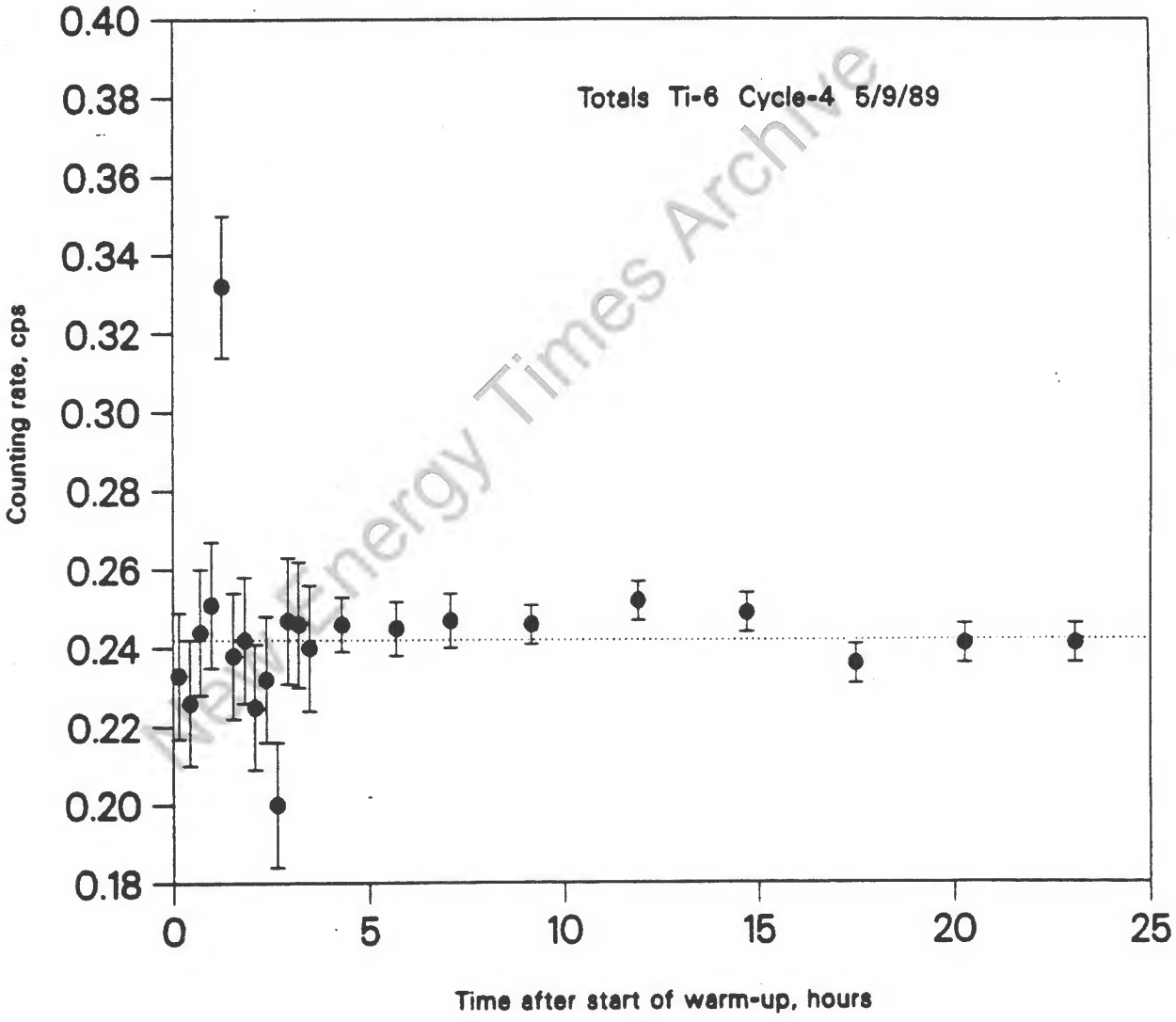
. BURSTS (Time Correlations)

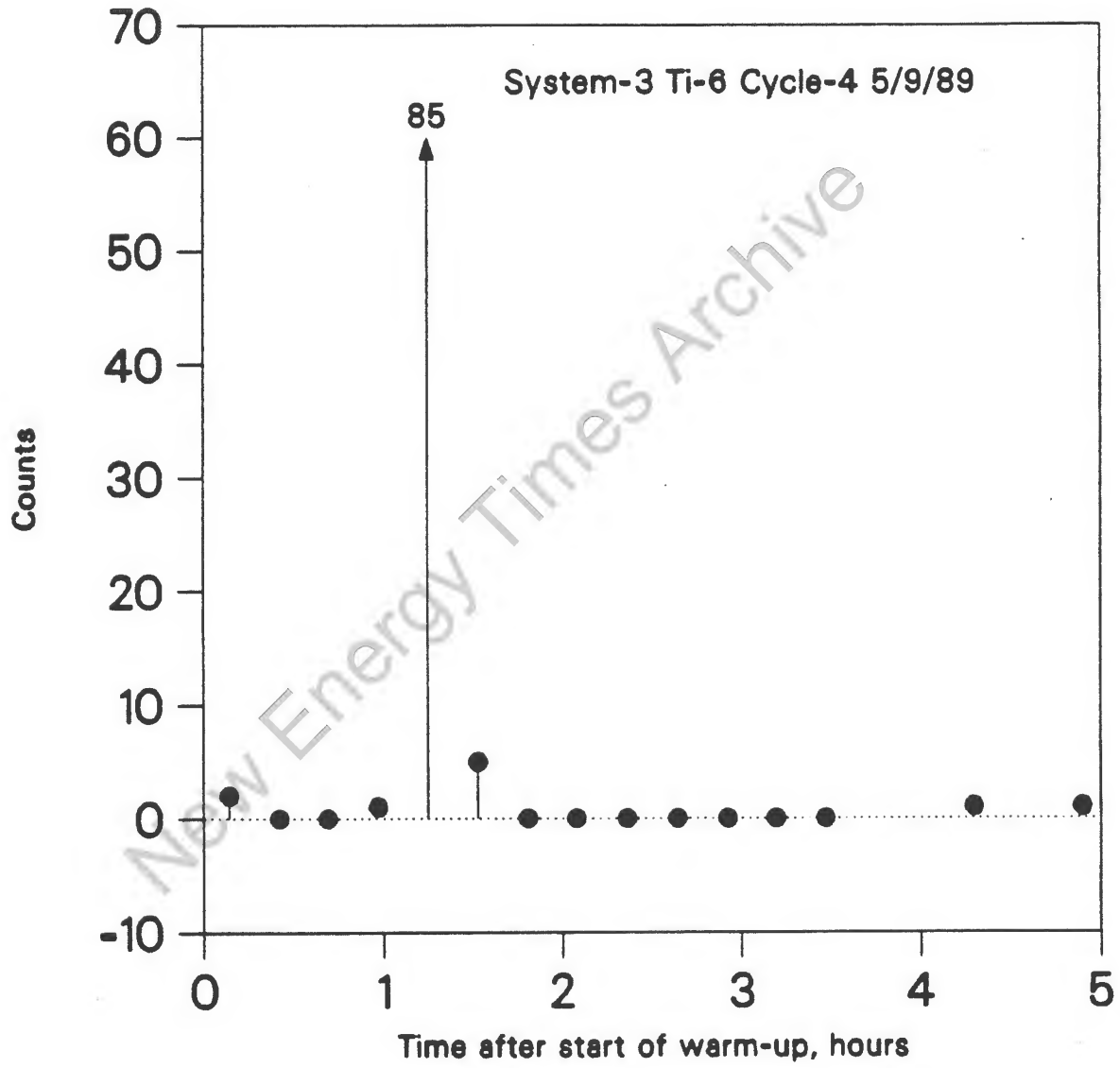




TIME CORRELATED COUNTING DATA FOR SAMPLE Ti-1







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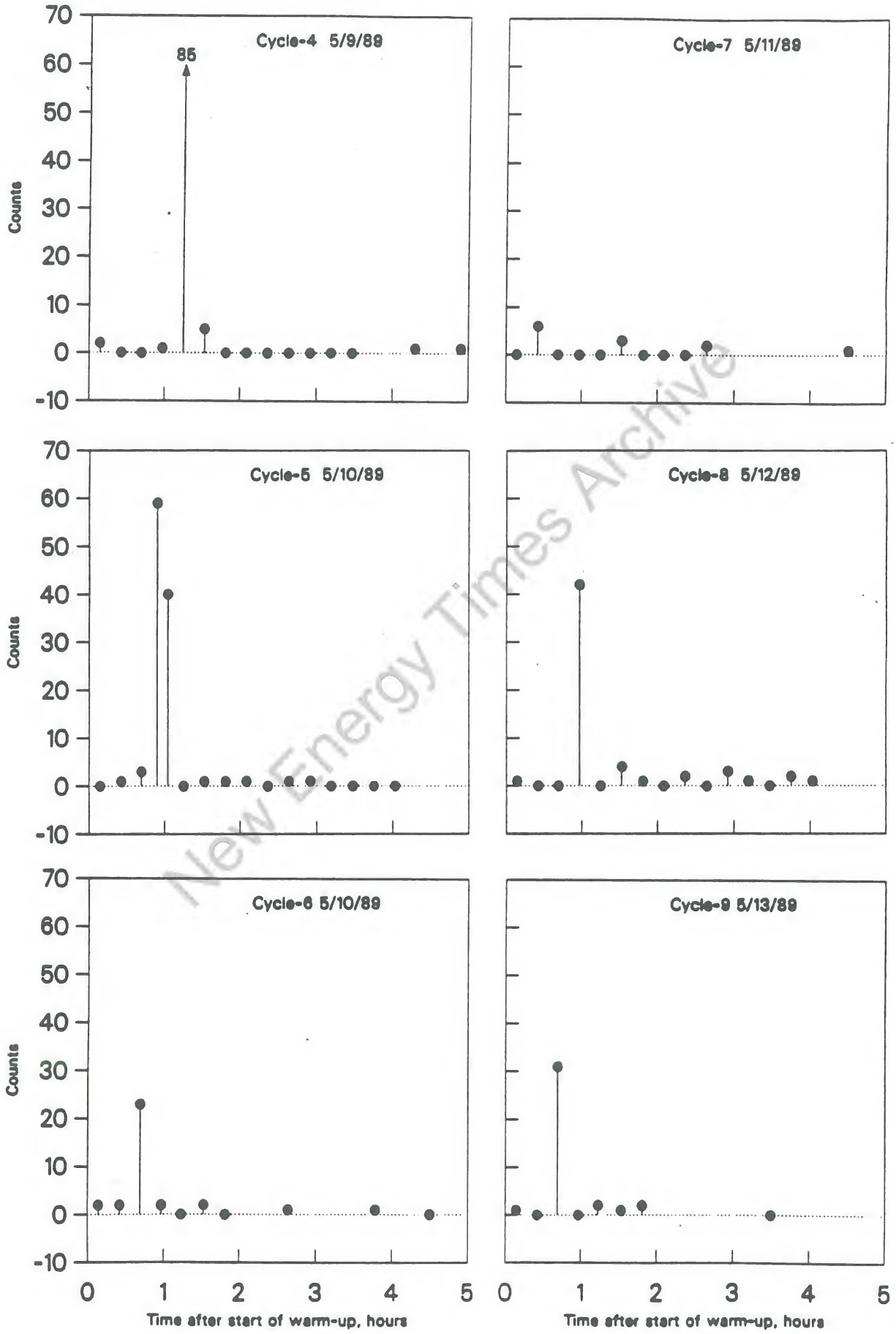
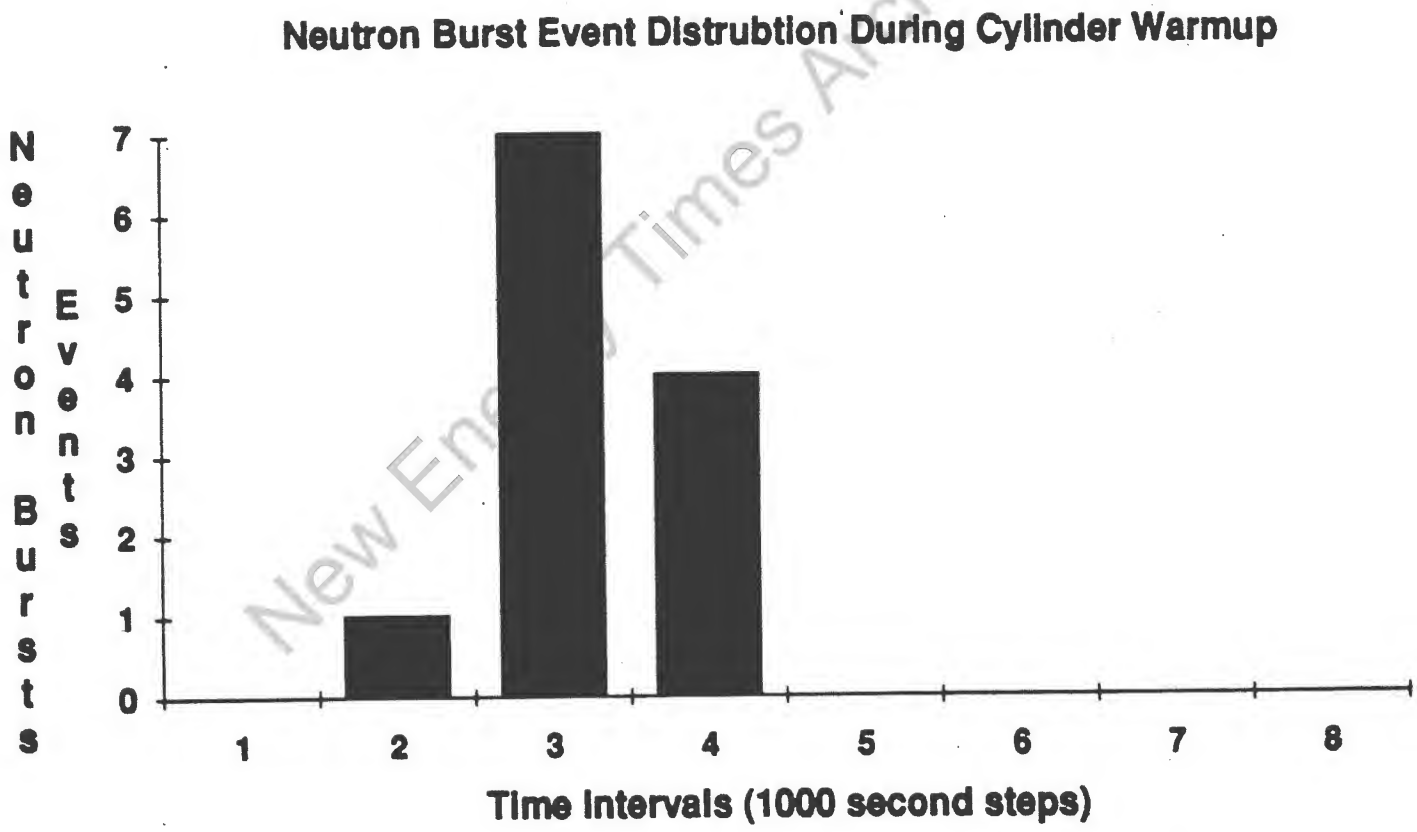
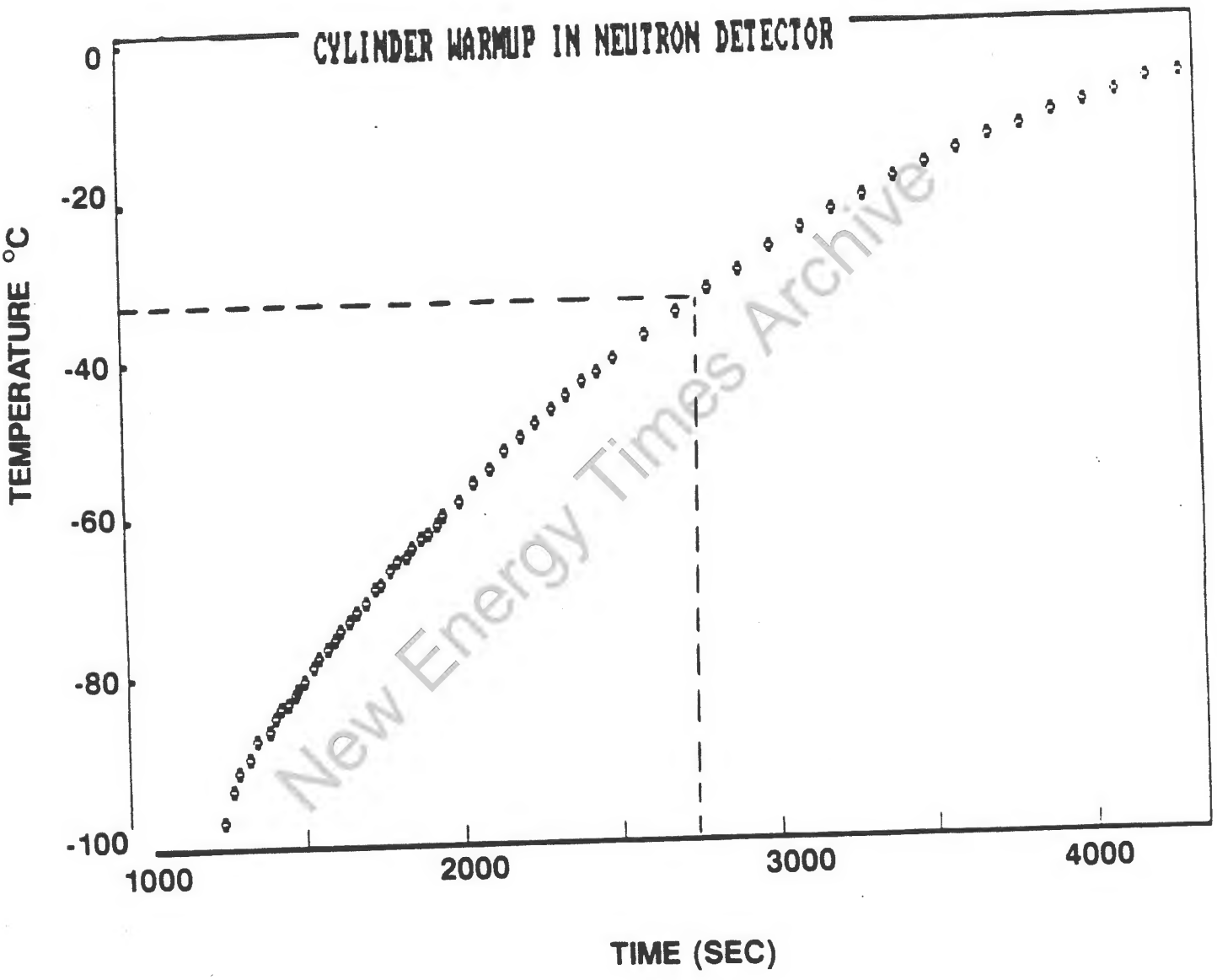


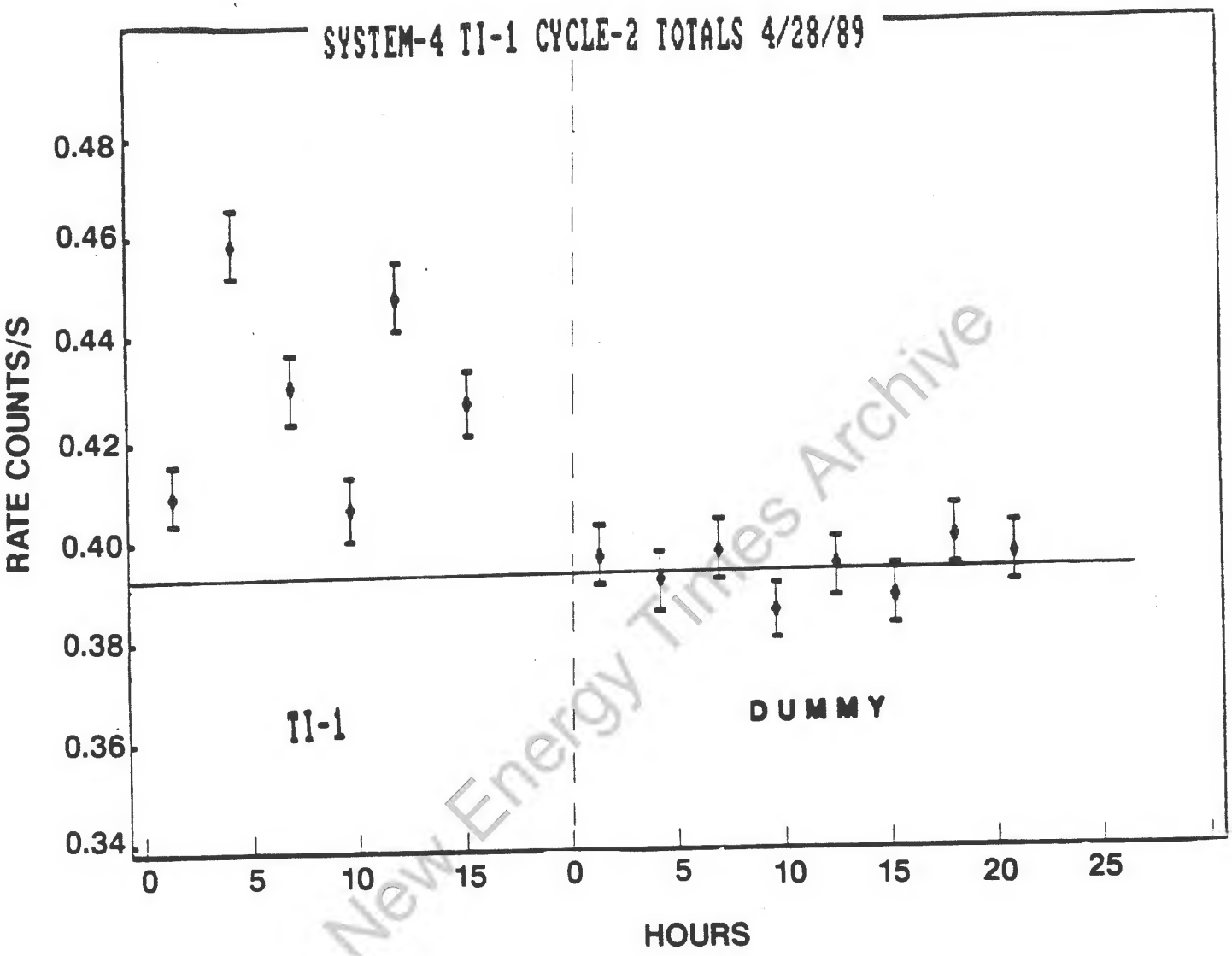


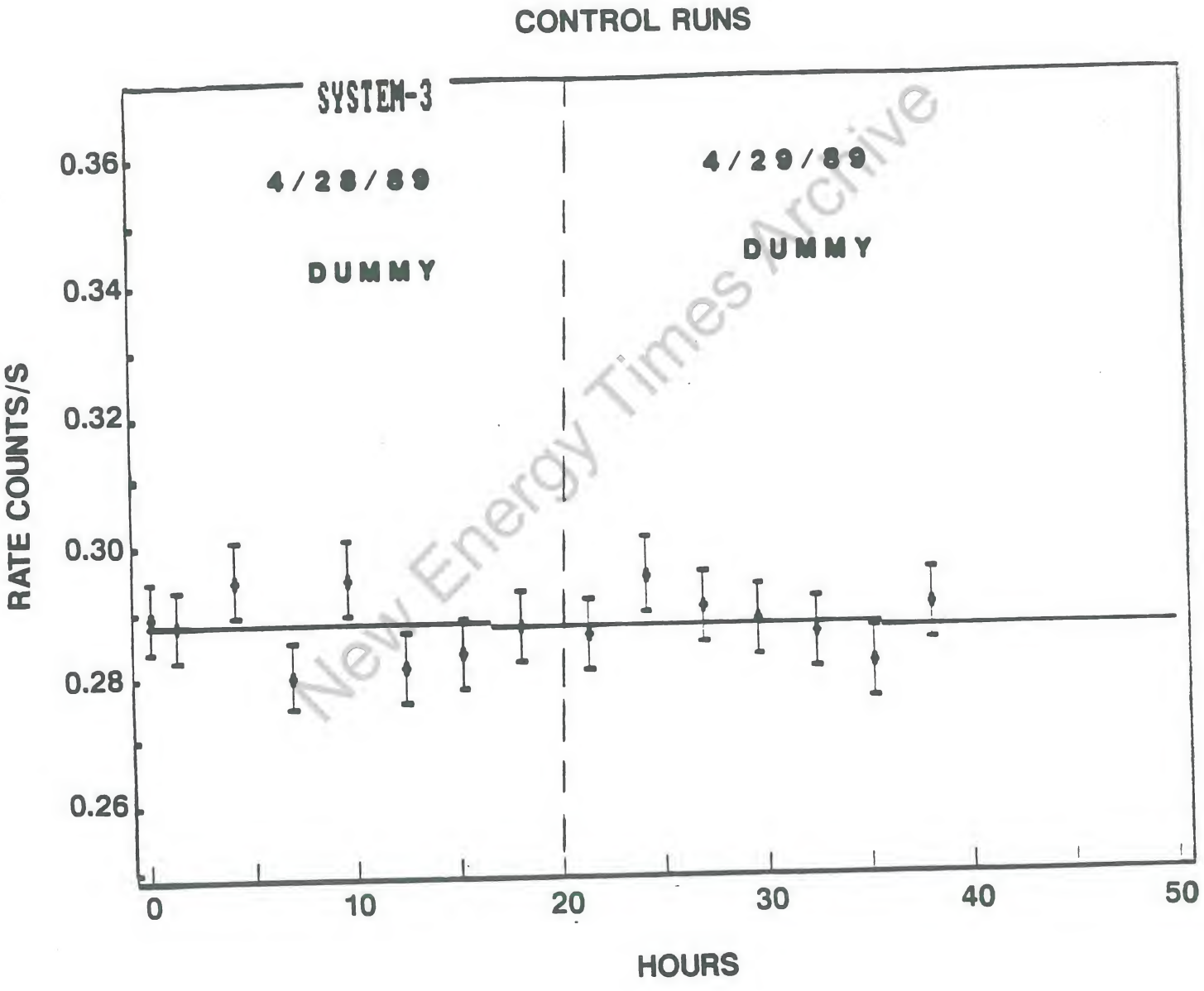


NEUTRON EVENTS

. RANDOM EMISSION

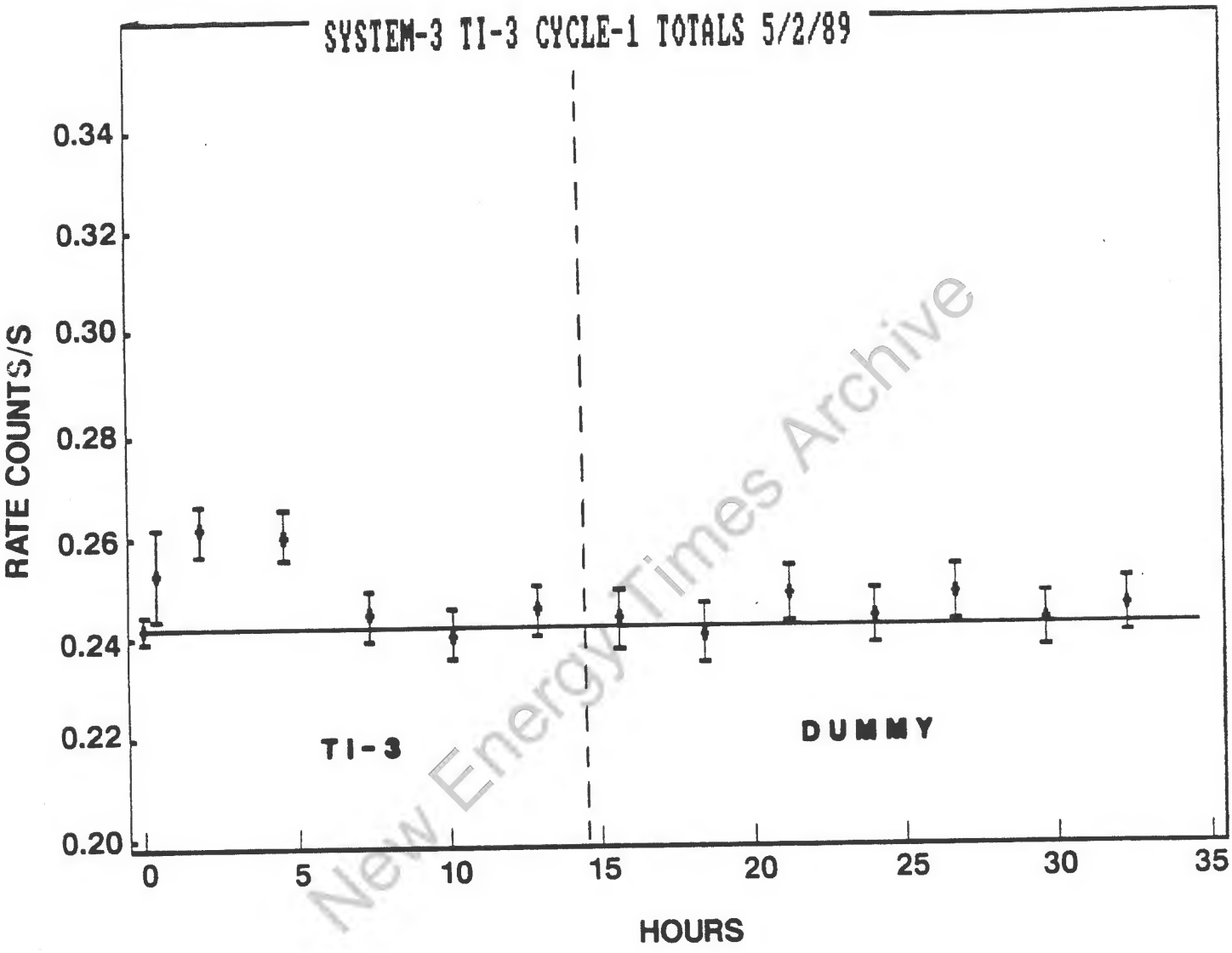
. BURSTS (Time Correlations)





RANDOM NEUTRON MEASUREMENTS
(Sample Ti-1)

Date	Measurement Time	Detector	Absolute Emission Rate	Standard Deviations
April 28	12 h	System 4	0.05-0.2 n/s	11σ
April 29	4 h	Systems 3 and 4	0.08 n/s	4.3σ



SENSITIVITY LEVELS

(3σ above background; 8 h count)

DETECTOR	LEVEL (n/s)
SYSTEM 1	0.05
SYSTEM 2	0.02
SYSTEM 3	0.03
SYSTEM 4	0.03

SUMMARY OF RESULTS

Neutron Burst Emissions

Sample	Number of Bursts	Burst Cycle No.
Ti-1	4	3,4
Ti-6	8	4,5,6,7,8,9
Ti-10	2	5,7
Ti-11	2	4

Random Neutron Emissions

Sample	Detector	Significance Level
Ti-1	System 4	11σ
Ti-1	Systems 3 and 4	4.3σ
Ti -3	System 3	5.3σ

SUMMARY

- **NEUTRONS HAVE BEEN DETECTED**
  - RANDOM NEUTRON EMISSIONS (0.05-0.2 n/s)
  - NEUTRON BURSTS (10-300 n)
  - BURSTS DURATION (≤100 μs)
  - BURST TEMPERATURE (~-30°C)
- **MECHANISM NOT IDENTIFIED**
  - COLD FUSION
  - "HOT" FUSION
  - OTHER?

FUTURE WORK

- MATERIAL CHARACTERIZATION FOR HIGHER YIELDS
- NEUTRON ENERGY SPECTRUM MEASUREMENTS
- MATERIAL STRUCTURE DETAILS
- GAS PHASE EXPERIMENTS WITH H AND T GAS

LA-UR 89-1974

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TITLE: MEASUREMENT OF NEUTRON EMISSION FROM Ti and Pd IN PRESSURIZED D_2 GAS AND D_2O ELECTROLYSIS CELLS

AUTHOR(S): H. O. Menlove, M. M. Fowler, E. Garcia, A. Mayer, M. C. Miller, R. R. Ryan, and S. E. Jones

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Los Alamos, New Mexico 87545

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MEASUREMENT OF NEUTRON EMISSION FROM Ti AND Pd IN PRESSURIZED D_2 GAS AND D_2O ELECTROLYSIS CELLS*

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ABSTRACT

We have measured neutron emission from cylinders of pressurized D_2 gas mixed with various forms of Pd and Ti metal. For some of the cases, the Ti was coated with a surface layer of Pd. The gas pressure ranged from 7 atm to 80 atm, and the Ti loadings ranged from 20 g to 200 g. Experiments also have been performed for D_2O electrolysis samples. The neutrons were measured using high-efficiency cavity-type detectors containing ^3He tubes. Random neutron emissions were observed as well as time-correlated neutron bursts. The time spread in an individual burst was less than 100 μs. For most of the samples, the neutron emissions were observed after the cylinders had cooled to liquid nitrogen temperature and were warming to room temperature. The bursts occurred about 40 minutes into the warm-up phase, and the random emission occurred for at least 12 hours after the sample reached room temperature. This cycle could only be repeated a few times before neutron emission ceased. The neutron emission rates were very low and the 12-hour random emission rate was 0.05-0.2 n/s. However, this yield was still 11σ above the background. The instantaneous neutron bursts were more dramatic with yields several orders of magnitude above the coincidence background rates.

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*Work supported by the US Department of Energy, Office of Safeguards and Security.

I. INTRODUCTION

The recent announcement by Fleischmann et al.,[1] that excess heat and substantial neutron flux had been observed in Pd cathodes in electrochemical cells, stimulated numerous experiments, although several claims (for example, gamma rays from neutron capture) have been retracted. Independently, Jones et al.,[2] observed 2.5-MeV neutron production at low levels during electrolytic infusion of D_2 into Ti and Pd electrodes, and discussed other means of creating nonequilibrium conditions, which might lead to "cold fusion." Subsequently, cold fusion has been reported in Ti subjected to pressurized D_2 gas and temperature changes.[3]. We now report neutron emissions at low levels in both electrolytic cells and in metals subjected to pressurized gases. In particular, using specialized methods, we have observed the production of ~10^2 n in bursts of ≤ 100–μs duration, as well as random neutron emissions.

As part of our investigations of the cold fusion phenomena and possible radiations from the samples, we have measured both burst neutrons and random neutron emissions from a variety of sample types. The samples included cylinders of pressurized D_2 gas mixed with various forms of Pd and Ti metal chips, sponge, crystals, and powder. In addition, we have performed neutron measurements for electrolysis cells containing D_2O and cathodes of Ti and Pd.

We are using four separate neutron detector systems operated in parallel experiments. The detectors all utilize ^{3}He gas proportional counters embedded in a polyethylene (CH_2) moderator. Three of the detectors are of the cavity- (well-) type, and one has an open channel for larger samples.

The electronics are based on shift-register circuits[4] that give both the random and time-correlated neutron counting rate.

II. DETECTORS

Four similar detector systems were used in the present experiments to increase sample throughput and to act as control experiments. All of the detectors were located in the same experimental laboratory with separation distances of 1 to 2 m. There was negligible crosstalk between the detectors because of the small yield of the neutrons and the large solid-angle coupling of each sample to its primary detector. The characteristics of the four neutron detectors are listed in Table I. The efficiencies were measured with a calibrated ^{252}Cf source (Av energy = 2.3 MeV). These compact high-efficiency detectors were developed at Los Alamos National Laboratory as part of the nuclear safeguards program.[5] Detailed descriptions of the detectors can be found in Refs. 6-8. These detectors are well suited for detecting the neutron burst emissions for the following reasons:

- The neutron slowing time jitter in the CH_2 gives an instantaneous burst of neutrons with a time spread of ~40 μs (neutron die-away time in the detector related to neutron moderation, leakage, and capture).
- Four amplifier channels are used in each detector with electronic clipping time constants of ~200 ns.
- There is a derandomizing buffer storage[4] at the input to the shift-register electronics to reduce deadtime for burst events.
- All neutron counts trigger the time correlation circuit without waiting through the gate time, thus reducing deadtime.
- High efficiency is required to detect a significant fraction of the small burst.

Figure 1 is a photograph of the System 3 neutron detector with a pressurized D_2 gas cylinder being placed in the cavity. The five CH_2 (^3He tubes) pods on the circumference of System 3 can be used to determine the average neutron energy when sufficiently high neutron yields are

obtained. The energy measurement is derived from the ratio of the counting rate in the outside tubes (^3He) to the inside tubes. The ratio is a function of the average neutron energy because of the differential energy transmission through the CH_2.

The time-correlated neutron counting[4] is essential for the neutron burst results reported in this paper. Every neutron count that enters the circuitry triggers the time-correlation counters that check if there are any other neutron counts within the selected time gate. We are using a coincidence time gate of 128 μs and this corresponds to about 3 times the neutron die-away time of the detector.

In addition to the coincidence gate scaler, there is an accidentals scaler that samples the same shift registers 1 ms after the trigger neutron count. Most electrical noise sources put counts into both the coincidence gate and the accidental gate, but in all of our observed burst events, the content of this scaler has been zero.

Additional measurements were performed to ensure that environmental noise was not getting into the detector systems. The results of these tests were as follows: (1) no gamma-ray sensitivity up to 1 R/h, (2) a detector efficiency temperature coefficient of only -0.01%/°C, (3) no electrical noise pickup for noise generators (Tesla coils) placed directly into the sample cavity, (4) a long-term stability (precision) of 0.01%.

III. MEASUREMENT PROCEDURES

A. Gas-Type Experiments

The Ti and Pd samples used in the D_2-gas-type experiments were contained in a pressurized gas cylinder approximately 5 cm in diameter and 25 cm long. After evacuating the sample for about one hour at 150-200°C, the cylinder was backfilled with D_2 gas at room temperature and sealed.

The sealed sample was then put through a liquid nitrogen (LN) temperature cycle. Typically, the LN cooling would last for 20-60 min. The sample was then removed from the LN and placed in the neutron detector cavity and allowed to warm to room temperature.

The measurement time bins were typically 1000 s; however, longer time intervals were used for some of the overnight runs. A complete temperature cycle would take about one day for most cases. Some cycles were shorter, lasting ~5 h. A given sample cylinder would be put through 7 to 14 of these cycles. The neutron emissions always ceased after a few cycles. Several of the samples were counted during the cooling down phase of the LN cycle with negative results.

One of the gas cylinders (DH-1) contained both D_2 gas (40 atm) and H_2 gas (40 atm) for a total of 80 atm.

B. Sample Material

At this early phase of the investigations, we have focused on experiments that contained a wide variety of material forms to maximize the chance of getting a neutron yield. The samples that gave neutron emissions contained mixtures of Ti and Pd turnings, sponge, foils, crystals, and powders. At least some of the material had been used in electrolysis experiments of the Jones-type.[2] Twelve gas cylinder samples have been used in the experiments and five have yielded neutron emissions. Our attempts to run experiments using a single material component to isolate the neutron source have been unsuccessful except for one case. This isolation is very difficult because of the nonreproducibility of the effect. That is, the "right material" category might still give a negative result because it lacked the special feature required for the emission of neutrons. The one exception to this isolation problem was sample DH-1 that contained only Ti alloy (6% Al, 6% V, and 2% Sn) turnings.

C. D_2O Electrolysis

In addition to the gas phase experiments, we have run four experiments using Jones-type[2] cells and electrolytes. Each of the experiments had six D_2O cells located in System 1. The anodes were gold foils and the cathodes varied with Ti, Pd, and V metal, foils, crystals, and sponge.

For one experiment, the electrolyte was D_2O mixed with the multiple ingredients described in Ref. 2. The other two included Pd and Ti in an acidified D_2O plus Li_2SO_4 solution. The currents

and voltages were also varied over the range described in Ref. 2. For the most recent (fourth) experiment, an electrolyte was used that contained only D_2O plus Li_2SO_4. The data were collected in 1000-s or 2000-s time bins, and each experiment lasted for several days.

During the electrolysis experiments, simultaneous D_2 gas-type experiments were in progress in Systems 3 and 4.

IV. RESULTS

A. Gas Phase Experiments

1. Burst Results. The first burst-type neutron emissions were observed from the gas-filled cylinder Ti-1. Most of the burst results give off too few neutrons to be measured in the uncorrelated (random) counting mode.

Cylinder Ti-1 underwent two LN temperature cycles without measurable neutron emissions. However, on cycle 3, two bursts were counted as shown in Fig. 2. The correlated background rate is about one count every 2000 s. Two more bursts were measured about 5 h later on temperature cycle 4. The dummy (inactive) cylinder was run through the temperature cycle alternating in the detector (System 3) with Ti-1. The control counter (System 4) provided null results during the entire experiment. Six subsequent LN cycles on sample Ti-1 gave null results.

Figure 2 gives the coincidence results for Ti-1, including the two active cycles as well as the results for the dummy cylinder that was cycled alternating with Ti-1 in System 3.

The next several cylinders were loaded with single Ti and Pd components in unsuccessful attempts to isolate the material that gave the neutron emissions. After four samples with null results, another integral (multicomponent) cylinder was measured with burst yields during six LN cycles (4, 5, 6, 7, 8, and 9). The largest burst results gave 85 neutron counts representing a source term of 253 n. This large spike was observable also in the totals rate.

A collection of the six active cycles with Ti-6 is shown in Fig. 3. Both Ti-1 and Ti-6 demonstrated a pattern of bursting during the third or fourth 1000-s time interval during the warm-up. The frequency distribution for the bursts from samples Ti-1 and Ti-6 peaked at 2800 s into the warm-up cycle.

To help establish the cylinder temperature vs the warm-up time, the dummy cylinder was run through the LN cycle with a thermocouple temperature probe inside the cylinder at the general location of the Ti material. The most probable temperature for the neutron burst observations was approximately -30°C. This burst time occurs about 15 min before the frost coating on the cylinders starts to melt.

The significance of the relationship between the temperature and the neutron bursts is yet to be established. It might be related to phase changes in the metal or to other stress and nonequilibrium conditions. The possibility of electrical discharge from cracking mechanisms will be discussed in the summary.

2. Burst Results from D_2 and H_2 Gas Experiments. In addition to the burst results described in the previous section, we have observed smaller bursts from a different type of experiment. In this experiment, the cylinder was loaded with 40 atm of D_2 plus 40 atm of H_2 gas. The addition of the H_2 gas was motivated by the possibility of obtaining p + d fusion in future experiments and measuring the high-energy (5.4-MeV) gamma rays.

This sample gave no bursts during the warm-up from LN temperature; however, we have observed at least four neutron bursts from the cylinder at room temperature. These data are shown in Fig. 4, where the top curve corresponds to the sample data and the bottom curve corresponds to the dummy cylinder run in the same detector (System 3). The control detector (System 4) was in operation during both the sample and dummy runs, and no bursts were observed in the control experiments.

3. Random Neutron Emissions. In addition to the burst-type results, we have measured random neutron emissions from two of the gas cylinders. The electronics that we are using were designed to separate purely random neutron emissions from time-correlated bursts in which two or more neutrons emitted at the same time are considered a correlated event.

Sample Ti-1 emitted random neutrons for at least an 18-h period while at room temperature. During LN temperature cycles 1 and 2, no burst neutrons were observed; however, when the sample was counted overnight (12 h) after cycle 2, we measured a yield of random neutrons. No time-correlation neutrons above the small background levels were observed during the 12-h period. Figure 5 shows the data from Ti-1 together with the data from the dummy cylinder collected the following night. The data taken in the control counter (System 3) during the same two nights gave a constant background rate during the two nights. The average of the sample data is 11σ above the average of the dummy runs.

Immediately following the Ti-1 overnight run, in System 4, the sample (Ti-1) was counted alternating between Systems 3 and 4 with the dummy in the opposite detector. These runs were for 4000 s each with two round trips in each detector for a total of 8000 s in System 3 plus 8000 s in System 4. Sample Ti-1 gave an excess of random neutron counts in both systems and the combined (both detectors) significance for Ti-1 was 4.3σ above the dummy background. No temperature cycling was involved during this period.

Sample Ti-3 gave an excess random neutron emission during the 5-h period following the first LN temperature cycle. The totals data are shown in Fig. 6. Subsequent temperature cycling of this sample gave no random or bursts emissions.

For the random neutron emission results, long counting intervals are required to statistically differentiate the low-level emission rates (0.05-0.2 n/s) from the cosmic-ray background rate.

B. D_2O Electrolysis Results.

We performed three experiments with Jones-type[2] cells where each experiment involved six D_2O cells containing different cathodes of Ti, Pd, and V metal. While two experiments showed

~3σ results above background levels, the limited sensitivity in the random-counting mode precludes any definitive statement concerning neutron emission at this time.

For a fourth experiment, we only used D_2O plus 10 g/L Li_2SO_4 with the pH adjusted to 4 with H_2SO_4. This experiment gave burst yields after running the current for about 12 h of electrolysis, and the bursts continued for several days. The time-correlation (coincidence) data from the detector (System 1) is shown in Fig. 7 (top). The burst activity continued for several hours after cutting off the current at 71 h into the experiment. The background dummy run (six clean D_2O cells) was made immediately after completing the sample run. These data are shown on the bottom portion of Fig. 7, and there is no evidence of burst events. The control runs taken in Systems 3 and 4 showed no burst activity during both the sample run and the dummy runs. The burst activity is evident for the sample run, and the largest burst corresponds to a source term of approximately 130 n. We are now trying to isolate which of the sample materials gave the neutron burst emissions.

V. SUMMARY

We have observed both burst and random neutron emissions from samples involved in seven different experiments. Table II gives a summary of the results. The significance level of the random neutron results is shown in the bottom of Table II. Two different detector systems have been used to measure the random emissions and three systems have detected the burst results. The individual burst results are as much as 2 orders of magnitude above the background levels.

The results reported in this work do not define the neutron production mechanism. Several models have been proposed for the production of neutrons in the two types of experiments for which they have been detected; that is, electrochemical experiments and those in which various forms of Ti metals or alloys have been subjected to thermal cycling under D_2 gas at pressure. The possibility of particle acceleration due to change separation during a fracturing process has been

suggested.[9-14] In support of the latter suggestion, both electron and positive particle emission have been observed at energies of several keV on a time scale consistent with our observed neutron bursts during the mechanical fracture of $TiD_{0.8}$.[14] Fracturing and fatigue of the samples has been observed in most cases where neutron emission has been detected in our laboratory.

The random emission results *cannot* be explained as a large number of small bursts because sample Ti-1 gave approximately 7000 random neutron counts during the 12-h experiment with no net correlations (including neutron pairs).

Detailed characteristics of the materials that give the neutron emissions have not been established. It is difficult to isolate the material characteristics that are responsible for the neutron emissions because of the unpredictability of the emissions. It is likely that the burst effect is not a property of the bulk material but it might be related to isolated dislocations in the samples. Also, there are probably different mechanisms responsible for the burst results and the random neutron emissions. The common denominator for all of the neutron emissions is that the samples are in nonequilibrium conditions.

We have not yet measured the neutron energies because the yields still are too low for the neutron spectrometry measurements. The neutron yields that we have measured in the present experiments are very low. For the highest random emission yields from Ti-1, the levels (0.1 n/s) were similar to the yields reported by Jones et al.[2] However, our neutron burst results are too small to be detectable in the uncorrelated (random) neutron counting mode. For example, a neutron burst of 40 n measured during a 5-h experiment represents an average source term of 0.002 n/s.

Our future work will focus on the characterization of the material to obtain higher yields and to understand the neutron source mechanism. We will measure the neutron energy and repeat the experiments with H_2 and DT gas to help establish if the neutrons are originating from cold fusion, hot fusion (cracking and fractures), or some other source.

ACKNOWLEDGMENTS

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FIGURE CAPTIONS

Fig. 1. Cylindrical detector System 3, consisting of 16 ^3He tubes in the central annulus plus 5 exterior CH_2 plus ^3He pods for background control and energy determination. A typical D_2 gas cylinder is being placed into the sample cavity.

Fig. 2. The combined coincidence results for the Ti-1 sample (left side) and the dummy cylinder (right side) that were measured in System 3. The dummy and the T-1 sample were measured alternating in time in the detector.

Fig. 3. The coincidence results for six active cycles for sample Ti-6 measured in System 3. The neutron burst results mostly occur 2000-4000 s into the warm-up period.

Fig. 4. The number of coincidence neutrons vs time for sample DH-1 (top curve) and the dummy cylinder (bottom curve). All of the bursts occurred at room temperature.

Fig. 5. The totals (random) neutron counting rate for sample Ti-1 measured in System 4 over a 12-h time period at room temperature. The right-hand section of Fig. 8 corresponds to data for the dummy sample in System 4.

Fig. 6. The totals (random) neutron counting rate from sample Ti-3 vs time for the warm-up from the first LN temperature cycle.

Fig. 7. The time-correlated (coincidence) neutrons counts vs time for six D_2O electrolysis cells measured in System 1. The bottom data correspond to six D_2O dummy cells measured in the same detector after the sample run was completed.

TABLE I

NEUTRON DETECTOR CHARACTERISTICS

Identification	Shape	Size	Number ^{3}He Tubes	^{3}He Pressure (atm)	Total Efficiency[a] (%)	Bkg (s^{-1})	Sensitivity Limit[b] (n/s)
System 1	Rectangular channel	25 x 35 cm	18	4	21	0.26	0.043
System 2	Cylindrical cavity	23 cm ϕ x 37 cm	6	4	26	0.10	0.022
System 3	Cylindrical cavity	22 cm ϕ x 35 cm	16	6	34	0.25	0.026
System 4	Cylindrical cavity	22 cm ϕ x 35 cm	16	4	31	0.39	0.036

[a]The total efficiency was measured using a calibrated ^{252}Cf source located at the sample position.

[b]Sensitivity limit corresponds to the source random emission rate for an 8-h count (3σ above background). For the burst type events, the sensitivity is about two orders of magnitude better.

TABLE II

SUMMARY OF RESULTS

Neutron Burst Emissions

Sample No.	Number of Bursts	Burst Cycle
Ti-1	4	3,4
Ti-6	8	4,5,6,7,8,9
Ti-10	2	5,7
Ti-11	2	4
DH-1	5	1,2
D_2O/Li_2SO_4	6	NA

Random Neutron Emissions

Sample	Detector	Significance Level
Ti-1	System 4	11σ
Ti-1	Systems 3 and 4	4.3σ
Ti -3	System 3	5.3σ

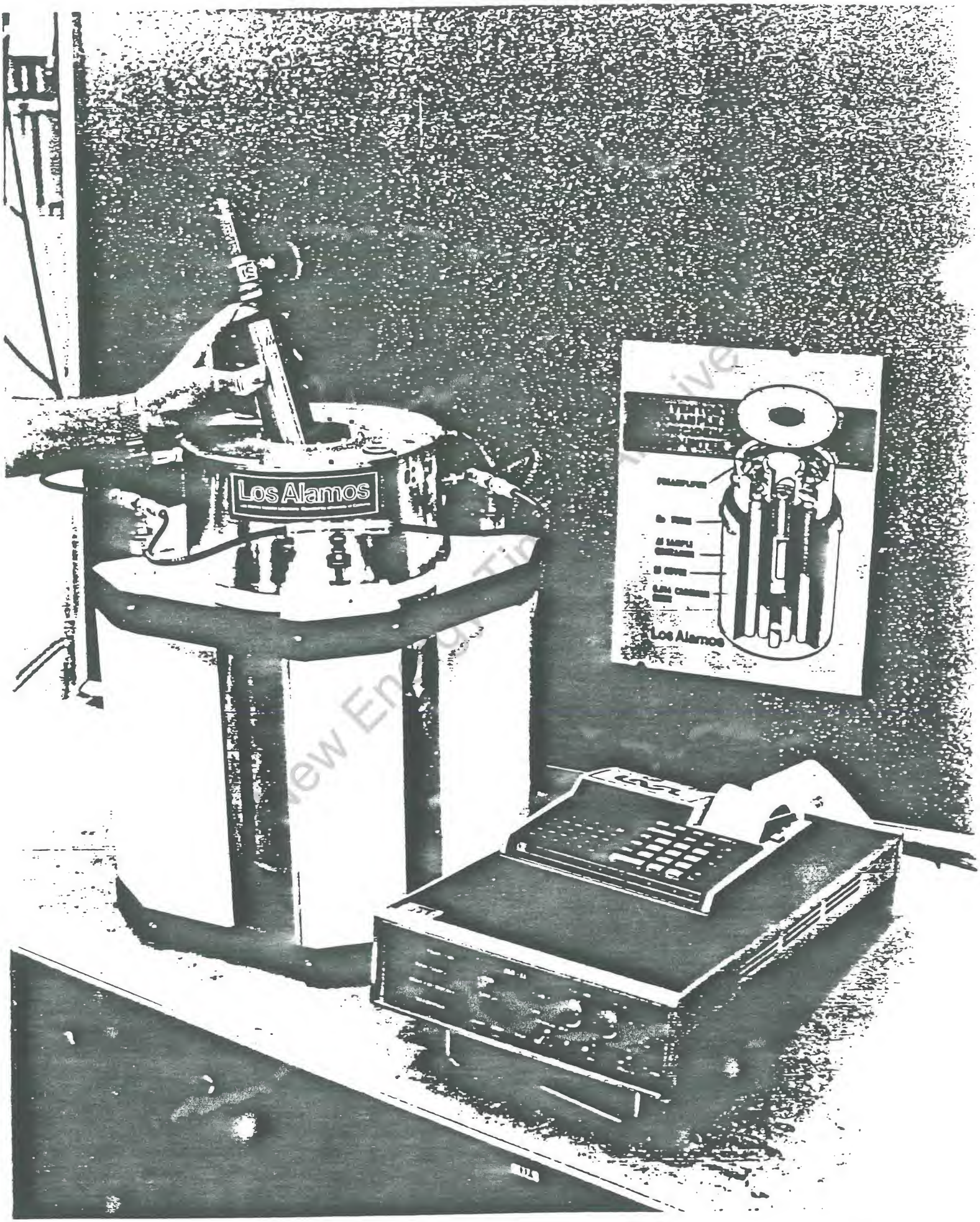
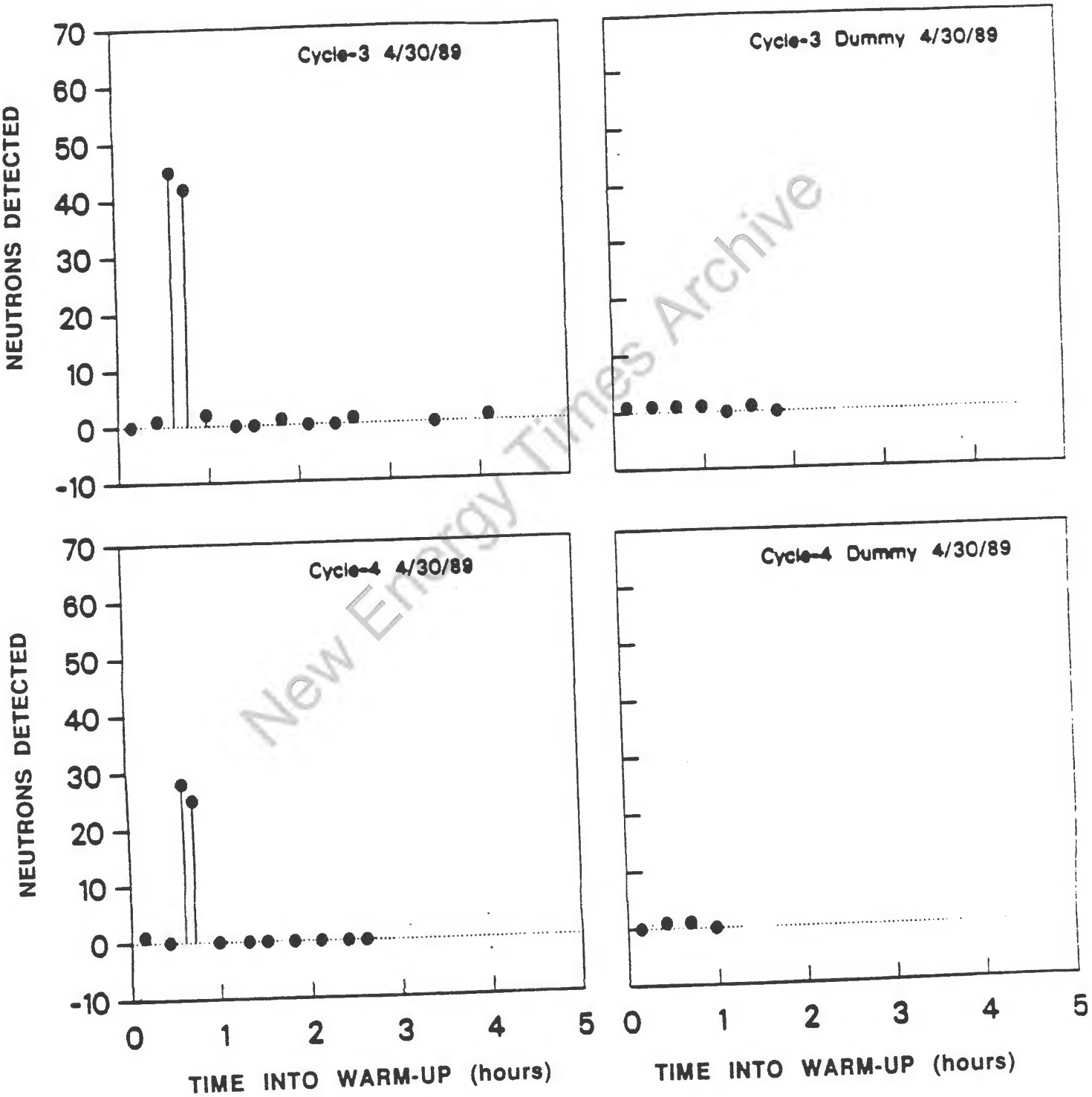
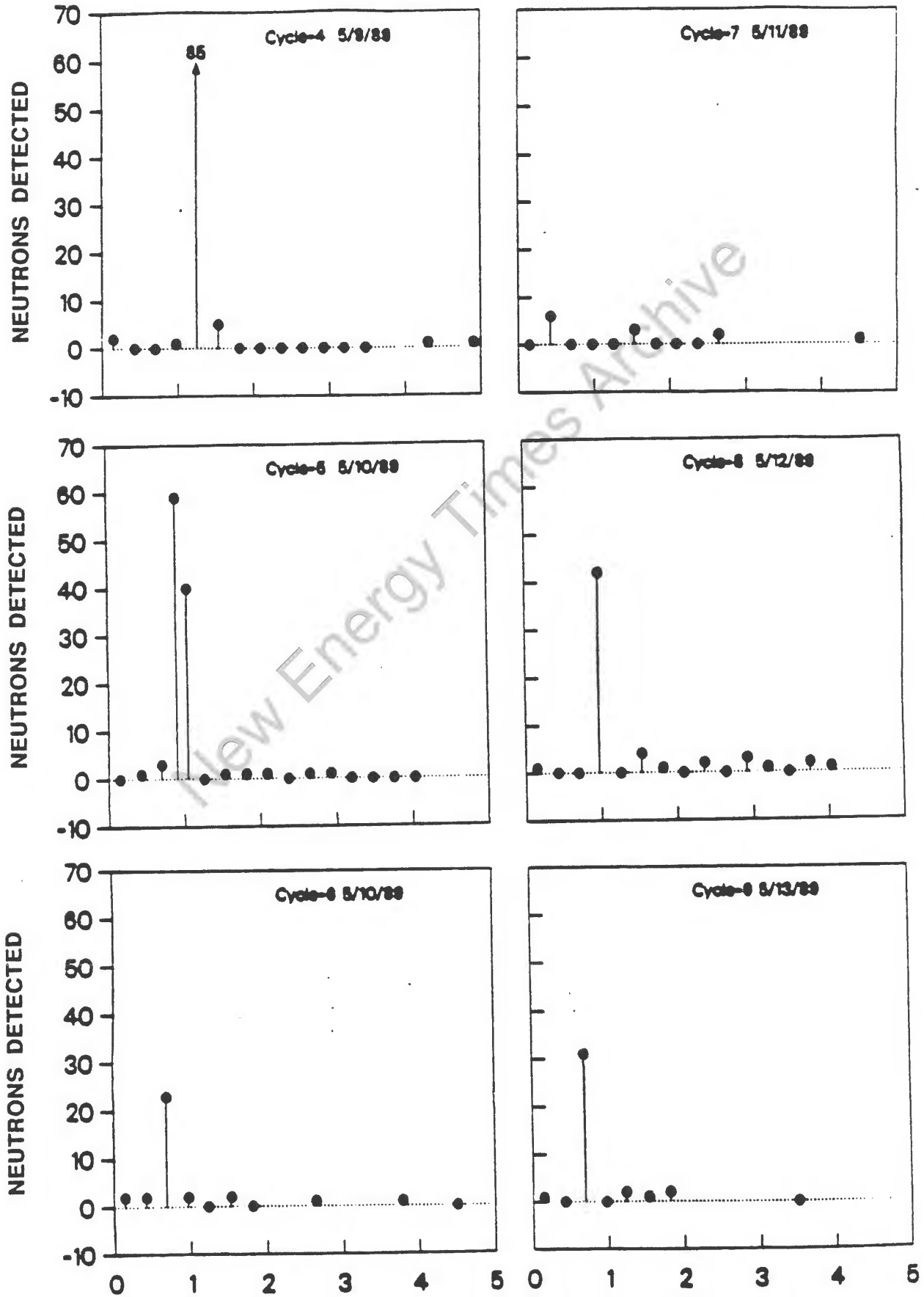


Fig. 1

TIME CORRELATED COUNTING DATA FOR SAMPLE Ti-1



TIME CORRELATED COUNTING DATA FOR SAMPLE Ti-6



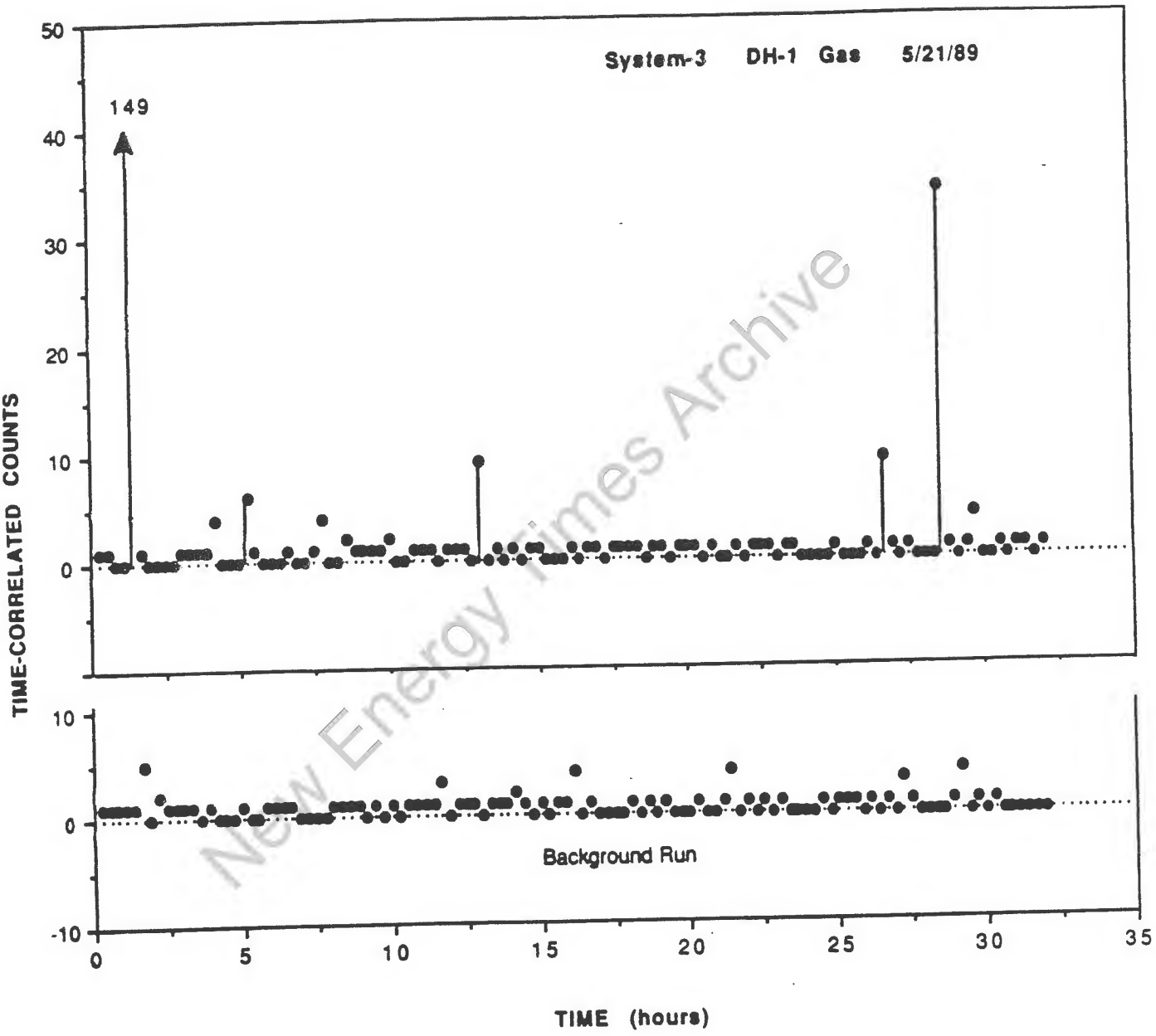
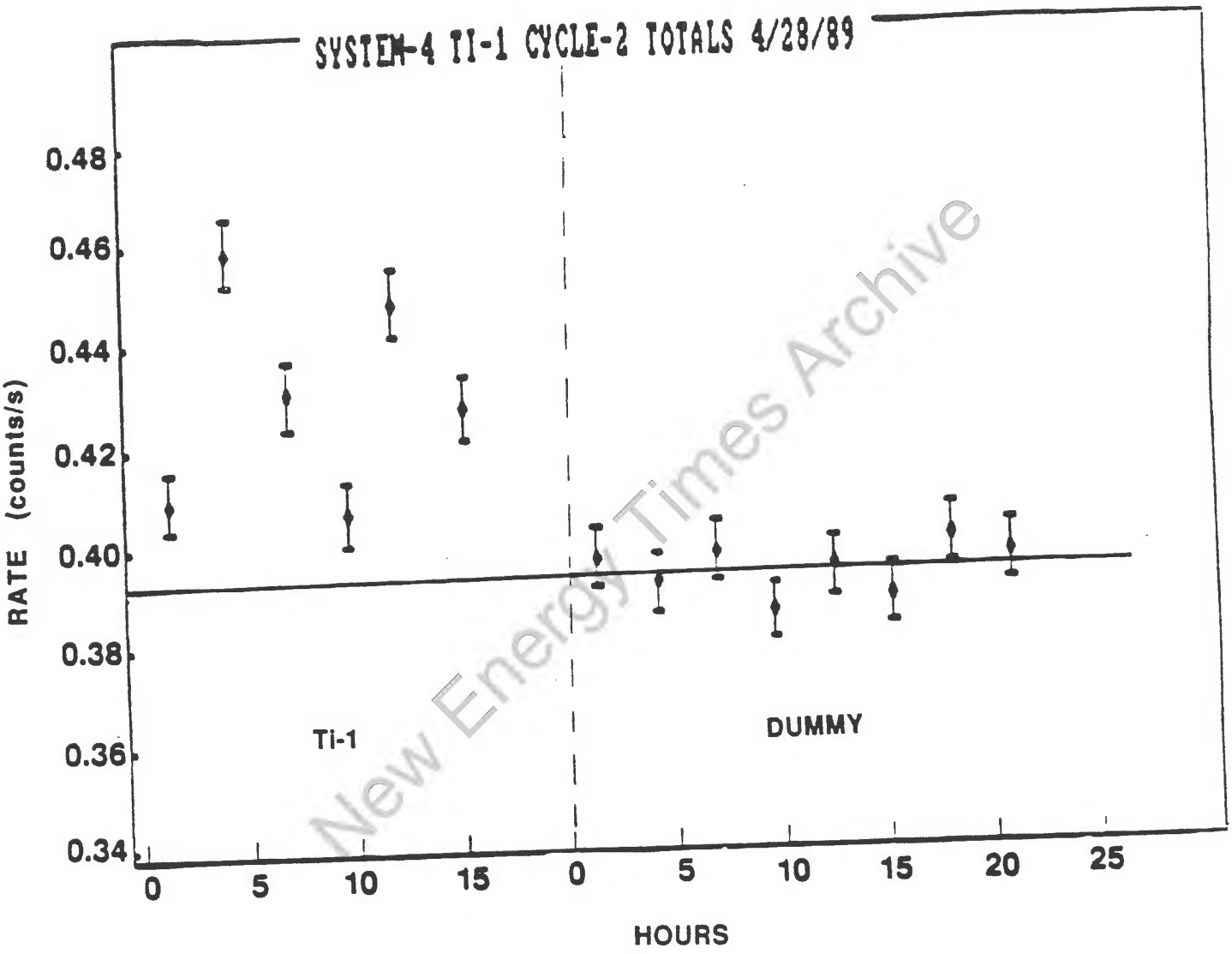
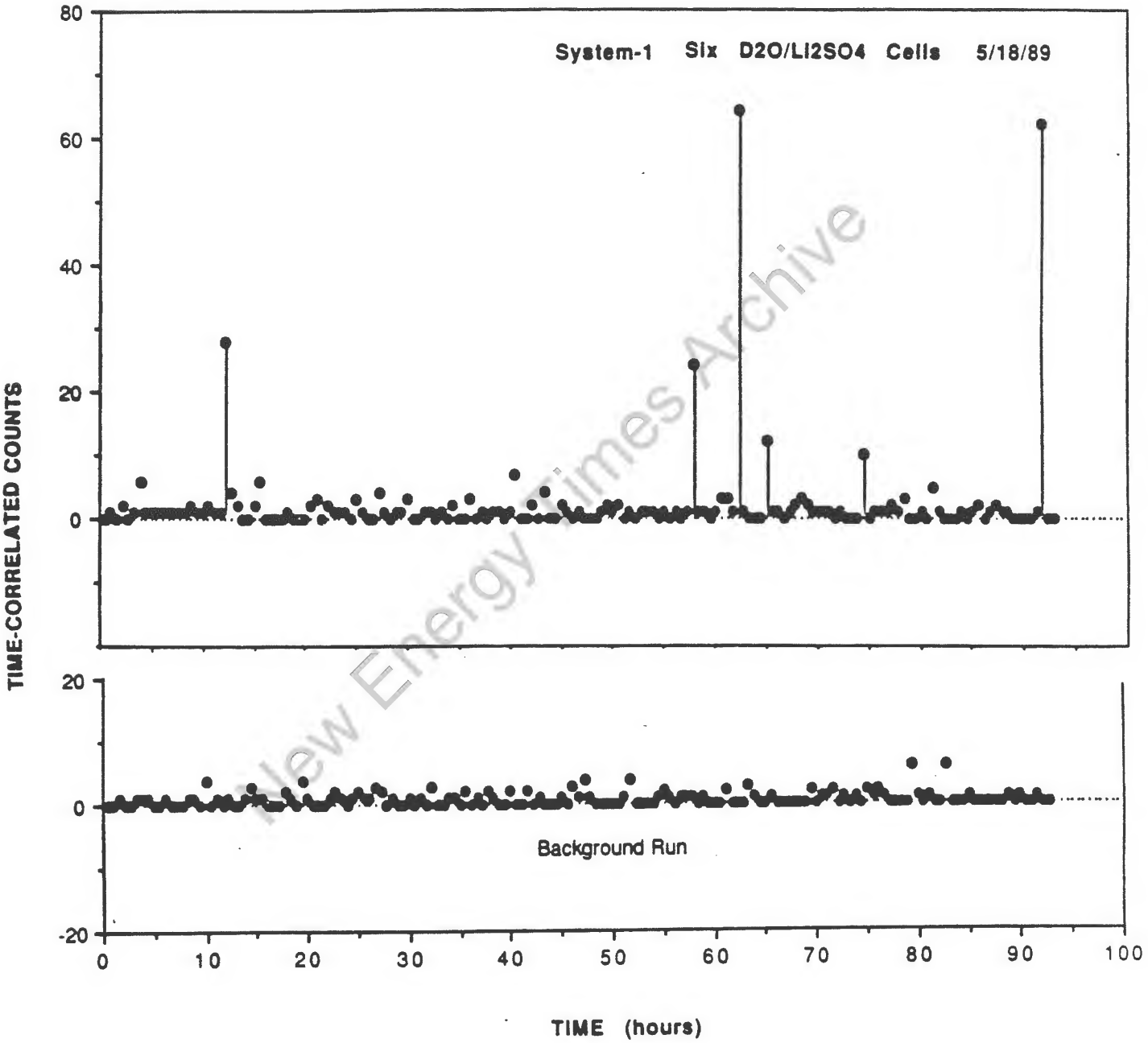
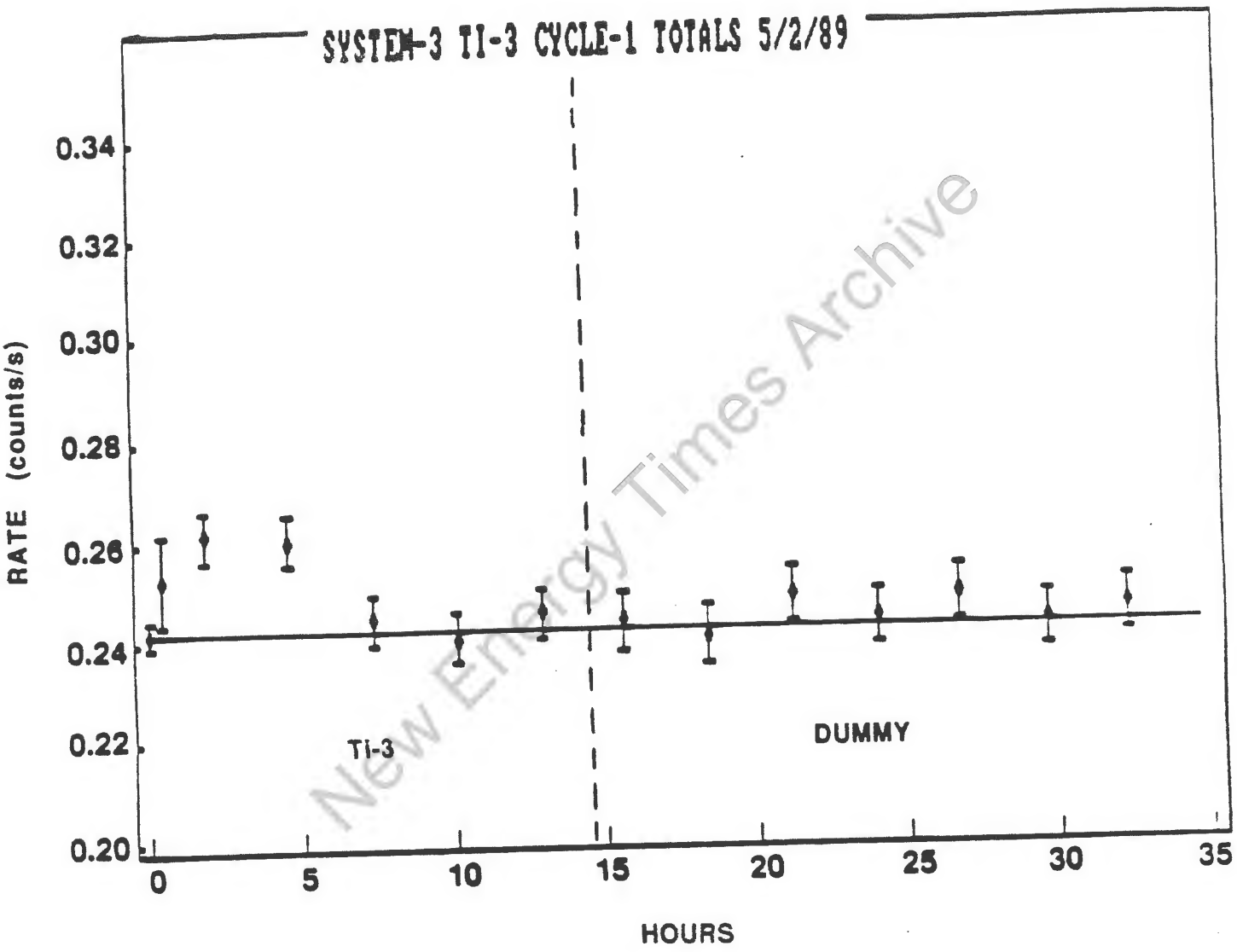


Fig. 4







Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

June 15, 1989

Conf. # 6362 7253

TO: Cold Fusion Panel

NYT A 1, 17; B 2, 6

Subject: Visit to Stanford University on July 6

Those who have indicated that they are attending are: Allen Bard, Howard Birnbaum, Clayton Callis, William Happer, John Huizenga, Darleane Hoffman, Kenneth Fowler, and Dave Goodwin and Tom Finn.

Reservations have been made for all participants for the night of July 5th (except Drs. Fowler and Hoffman) at the Holiday Inn, 415 El Camino Real, Palo Alto, CA. Their telephone number is (415) 328-2800, and each of you should call the Inn and give them your credit card number to confirm your reservations. You have a government rate of $56.65 (including tax) and be sure to bring your travel authorization with you if they should ask for it.

Your meeting will begin on July 6th at 8:30 a.m. with Dr. Robert A. Huggins at his office, which is on the Ground Floor of the Peterson Building (Building 550) at Stanford. A map is attached.

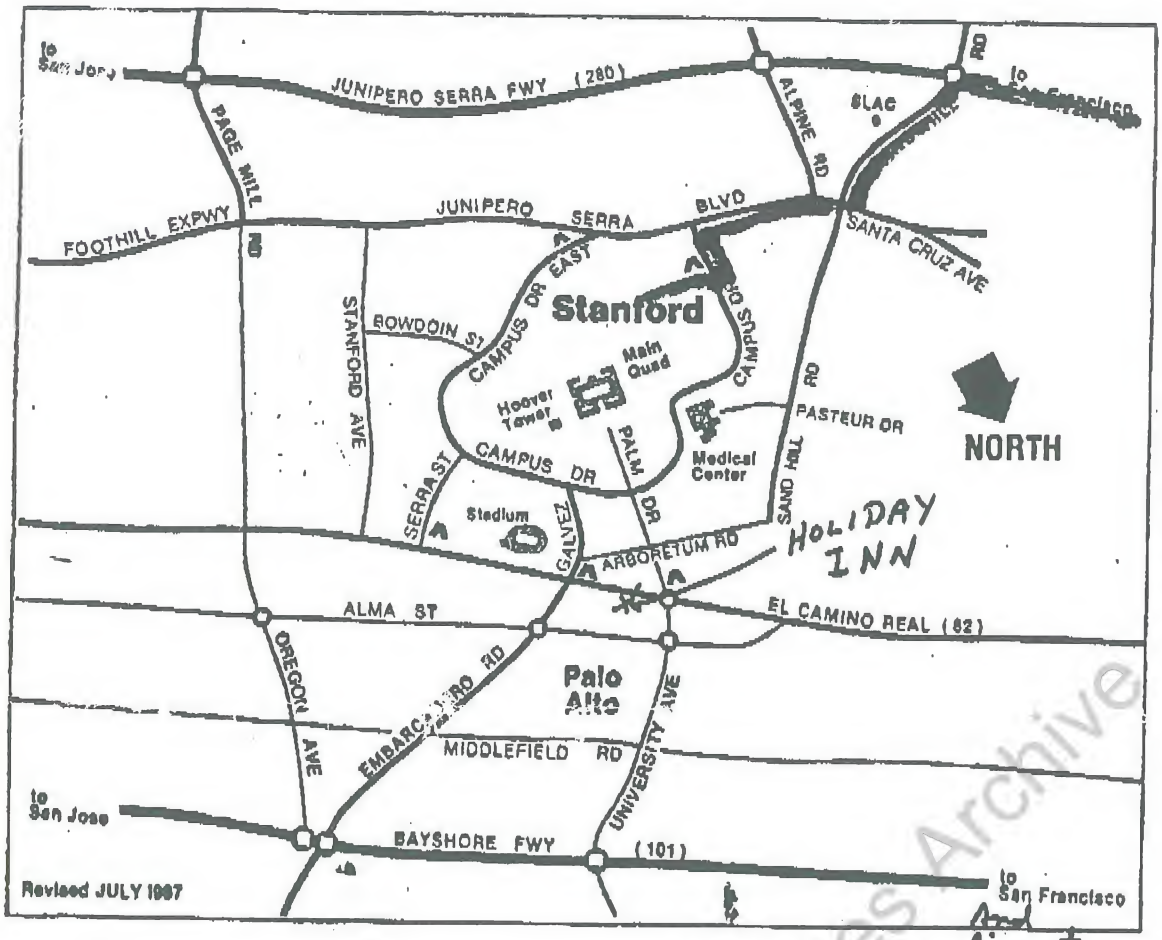
I suggest you rent cars, possibly sharing rides with individuals with your approximately travel schedules.

Other members who wish to make this visit should let me know as soon as possible.

William Woodard

Attachment

3-1133 WE

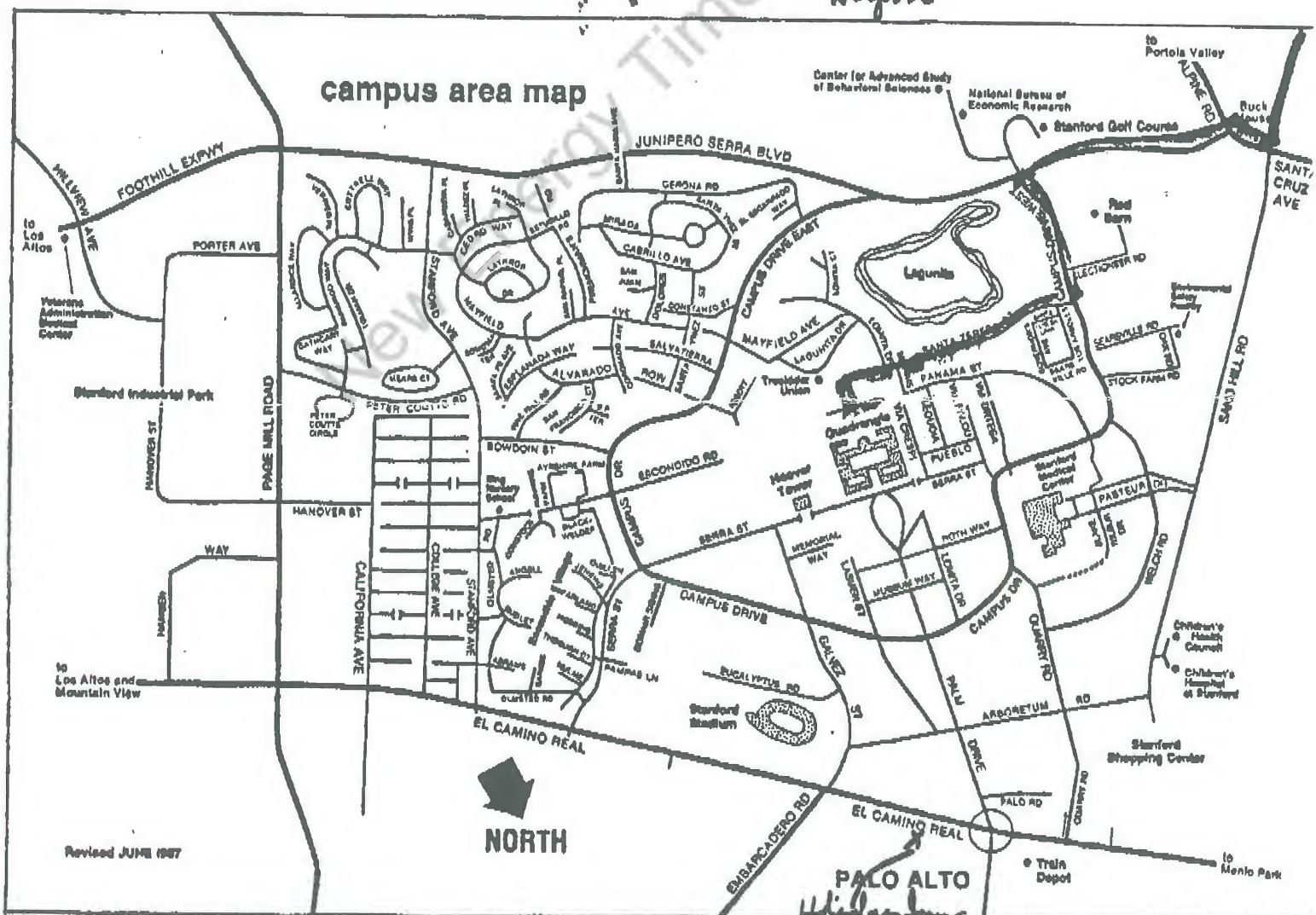


STANFORD UNIVERSITY · ORGANIZED 1891 ·

Approaches to the Stanford campus from freeways 101 and 280

Stanford University is on the "Peninsula," in the county of Santa Clara, approximately 30 miles southeast of San Francisco and 20 miles northwest of San Jose.

▲ Location of campus directory



STANFORD UNIVERSITY

NORTH

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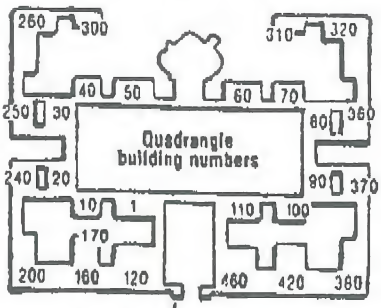
METER or PAY LOT Parking: 8 to 5, Monday through Friday

Disabled Person Parking. For use only by persons with disabled person permits. Enforced at all hours every day. $50 minimum fine for parking in these zones without proper permit.

Parking permits are required in 'A' and 'C' lots from 8 to 5, Monday through Friday. At other hours, these lots are free and open to the public.



Information for visitors
Robert A. Millikan Library
Peterson Lane (off Bldg 550)
Room 550 I
10am 7/6/89
(415) 723-4181

AUDITORIUMS and AMPHITHEATERS numbered on map

1 — Annenberg — Cummings Art Building
2 — Bolin — Braun Music Center
3 — Bishop — Graduate School of Business
4 — Braun Aud — Mudd Chemistry Building
5 — Braun Rehearsal — Braun Music Center
6 — Campbell — Braun Music Center
7 — Cubberley — School of Education
8 — Dinkelspiel
9 — Elliott
10 — Fairchild — Fairchild Center
11 — Frost
12 — Jordan — Bldg 380 Quad
13 — Kresge
14 — Little Theater — Memorial Hall
15 — Maples
16 — Memorial — Memorial Hall
17 — Newby, Ying — Bldg 380
18 — Princeton Lecture Hall
19 — Schultz — Health Research & Policy Bldg
20 — Skilling
21 — Stadium
22 — Stauffer — Harmony Memorial Bldg
23 — Sunken Diamond
24 — Terman — Terman Engineering Center

TICKET OFFICES

27 — Tresidder Memorial Union

MARGUERITE shuttle services (FREE): All times Monday through Friday only

BLUE and PURPLE lines circle Inner campus 7:20 am to 6:15 pm and connect to Stanford Shopping Center 11:30 to 2

GREEN line circles Palo Alto Caltrain, Stanford Medical Center, and Quadrangle during am and pm commute hours.

SILVER and GOLD express lines travel from Stanford Medical Center and Quadrangle to downtown Palo Alto 11:30 to 2

SLAC line travels from Sequoia Hall, on Santa Fe, to SLAC with one stop at Campus Drive West and Roosevelt Rd 8:30 to 5:30.

For specific information on all the above, call the Office of Transportation Programs at (415) 723-0383.

HOLIDAY INN

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

June 28, 1989

TO: Cold Fusion Panelists and Staff Visiting SRI (Bard, Birnbaum, Callis, Garwin, Happer, Huizenga, Hoffman, Fowler, Goodwin, and Finn)

SUBJECT: July 6 Visit

Richard Garwin has arranged for you to meet with Michael McKubre at SRI on July 6, following your meeting at Stanford. Your appointment with Dr. McKubre is at 1:30 p.m. so you should probably plan to leave Stanford by 1:00. SRI is located in Menlo Park at 333 Ravenswood Avenue. When you leave the Stanford campus take El Camino Real (north) about 4 traffic lights to Ravenswood Avenue and turn right. Go about a mile on Ravenswood and look for a sign on the right for "SRI Visitors". A receptionist will call Dr. McKubre and he will meet you. For future reference his address and telephone number are as follows:

Dr. Michael McKubre
Room AB 222
SRI International
333 Ravenswood Avenue
Menlo Park, CA 94025
Telephone (415) 859-3868

Bill
William L. Woodard

Bye Bill
Fondest
Regards
Mike

cc: Michael McKubre
Tom Passel, EPRI

855 - 2070

EPRI/NPD

OPENING REMARKS
T.O.PASSELL

- THIS IS A PROJECT REVIEW AT A EARLIER STAGE THAN IS USUAL TO ACCOMODATE THE VISIT OF ADM. WATKINS' COMMITTEE INVESTIGATING THE STATUS OF COLD FUSION RESEARCH.
- THIS ELECTROCHEMICAL CELL RESEARCH WAS INITIATED TO CONFIRM OR REFUTE THE CONNECTION BETWEEN HEAT AND NUCLEAR FUSION EVENTS IN PALLADIUM CATHODES AS REPORTED BY PONS AND FLEISCHMANN
- EVEN THOUGH NO CONCLUSIVE RESULTS ARE YET AVAILABLE FROM THIS EXPERIMENT, I FELT IT WAS USEFUL TO EXPOSE THE CONCEPT AND PROCEDURES BEING FOLLOWED BY THE SRI INT'L RESEARCHERS TO A WIDER AUDIENCE BECAUSE OF THE UNIQUE APPROACH
- THIS INFORMATION IS BY NO MEANS READY FOR PUBLICATION OR THE BASIS FOR ANY CLAIMS THAT COLD FUSION HAS EITHER BEEN CONFIRMED OR REFUTED.

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

June 12, 1989

Professor Norman F. Ramsey
Harvard University
Lyman Laboratory of Physics,
Room 228
9 Oxford Street
Cambridge, MA 02138

Dear Norman,

As promised, I spent Friday afternoon, 06/09/89, at Frascati, looking into the experiment by F. Scaramuzzi, et al. Scaramuzzi had to be away, so he arranged for me to talk with Marcello Martone. I had a full exposure to the table-top experiment. My comments are summarized in the enclosed letter to Martone.

Martone now understands that efforts to maximize the yield by variations in the conditions of the experiment have been misguided; he will now concentrate on attempting to reproduce the initial results.

Martone gave me details of another experiment on which we had only sketchy information by Scaramuzzi in Santa Fe. As a result, I have written the enclosed MEMO to my colleague at IBM, Jim Ziegler.

Norman, I enclose also Cold Fusion News, No. 14 from Douglas R.O. Morrison (CERN). I think this is a very useful report of the Santa Fe meeting. I am sending it along with this letter and other attachments to Bill Woodard and urging prompt distribution to the members of the committee.

Although I have been in Europe the last week, I am disturbed by the appearance in the newspaper of comments by Panel members on views of other Panel members. It seems to me that Panel members should not ascribe views to others on the Panel. Furthermore, I reject the allegation of Stanley Pons that I am "negatively biased" in my views. I actively seek experimental results, whether they show evidence of neutrons or heat, or not. I assess their validity, and modify from day to day my judgment as to what is going on. If you would accept the view that a Panel member is "negatively biased" if he or she judges that their is no solid evidence of heat produced in the Utah experiments, what would happen if the entire present Panel came to that conclusion? Would we be

forced to add still more people who believed that their was heat, in order to maintain a "balance"?

Thus far, my favorite experiment is that by M.E. Hayden, "High Precision Calorimetry ..." Vancouver, simply because of the evident care with which the experiment was designed and performed, and the clarity with which it was presented.

As for the experiments by H.O. Menlove, et al (Los Alamos), I have heard that engineers working on the experiment suggest that the "bursts of neutrons" observed are due to sensitivity of the counters to ultrasound. I spoke with Howard Menlove from Rome 06/08/89, who says that the counters have no sensitivity to ultrasound, but that they were going to install a microphone on the sample container to look into the matter. I shall communicate by telephone with these individuals 06/13/89 to see what I can learn.

I am particularly looking forward to my visit to Robert Huggins at Stanford University.

Very best regards.

Sincerely yours,

Richard L. Garwin

Encl:
06/12/89 LTR RLG to M. Martone re summary of 06/09/89 visit.
06/09/89 MEMO RLG to J. Ziegler re cold fusion.
05/28/89 Cold Fusion News, No. 14, report of Santa Fe meeting. (052889DROM)

cc:
> John Huizenga, Rochester.
> W.L. Woodard, ERAB.

RLG:jah:163%NFR:061289.NFR

To: James F. Ziegler
28-024
P O Box 218, Yorktown Heights, NY 10598

Date: June 9, 1989
From: R.L. Garwin x2555 26-234 Yorktown Heights, NY
IBM Fellow and Science Advisor to the Director of Research.
VNET: RLG2 at YKTVMV;
RSA1 at YKTVMV (JoAnn T. McLoughlin, Secretary)

Sub.: Cold fusion again.

As I briefly heard in Santa Fe, there has been an experiment done by folks of ENEA at Casaccia-- G. Mazzone and M. Vittori. These people took 10 grams of Ti foil, 0.5 mm thick. They degassed at 1000 C and then they applied 20 Torr of deteurium for 20 minutes, cooling to room temperature. Then they suddenly heated to 1000 C with a lamp at a very high rate-- perhaps 100 C/s. While they heated, they pumped out the gas. Then they had neutrons for 5 minutes, with a peak rate of 7000 counts per minute. With an efficiency of the counter of about 0.5%, this was a source rate of 1 million per minute, peak.

In a second run with the same Ti sheet, they degassed at 1000 C, cooled to RT, then admitted 20 Torr of D2. They observed neutrons for 5 min at a peak rate of 1800 counts/min.

In a third run, after degassing at 1000 C they held the sample at 500 C while soaking in 20 Torr of D2. On heating rapidly to 1000 C, they saw 7000 counts/s of neutrons peak (for 5 minutes), but this time there was also a long (one hour) low tail.

This is sufficiently quick and clean that I think we should do it. Also, no high pressure is involved-- could do it in a quartz tube with radiant heater or ordinary furnace. You might ask Thjs Broer whether Bell has done anything along these lines.

R.L. Garwin

cc:
P.M. Horn, YKTN.

RLG:jtml:160%JFZ:060989.JFZ

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555
FAX: (914) 945-2141, Telex: 137456 IBMRESRCH YKHG UD,
BITNET: RLG2 at IBM.COM

June 12, 1989
(Via FAX to (39-6) 9400-5400)

Mr. Marcello Martone
ENEA
Dip, TIB, U.S. Fisica Applicata
Centro Ricerche Energia Frascati
C.P. 65-00044 Frascati
Rome
ITALY

Dear Dr. Martone:

Thank you very much for the frank and warm discussions and for your hospitality in receiving me in your laboratory Friday, 06/09/89. I was very glad to see the actual apparatus and also to talk with Dr. De Ninno.

When I first saw and obtained a copy of your preprint from Professor Amaldi 04/23/89, I was, of course, most interested to understand it. So I was glad to be able to discuss with you extensively Figs. 2 and 3 of that report and to be able to see the actual log book.

You were interested in my views as to what might be done to validate these results, and I am happy to set them forth below. I think it very important, as we discussed, to try to repeat the successes of this experiment, rather than to use our "understanding" (of which we have very little) to optimize the yield.

First, there is a set of experiments that will help to determine whether the counter itself is responsible for these signals, completely independent of the treatment of the sample. To this end, the counter ought to be left on continuously (or when not left on continuously, its counts should be recorded whenever the counter is on), with a view to seeing whether, over a long period of time, there are any bursts of counts such as are shown in Fig. 2, or any indication of signals such as those shown in Fig. 3.

Dr. Douglas R.O. Morrison, of CERN, has been distributing his weekly assessment of the situation in Cold Fusion News. No. 14 of 06/01/89 reports on the Santa Fe meeting, and on pages 8 and 9 reports as follows:

> "Moshe (Gai) also gave a short evening talk about possible errors that can occur. Firstly he said that BF3 counters should not be used! - they were too unreliable and subject to false counts from temperature changes, humidity, vibration.... Also PSD could cause errors and calibration was necessary."

If you or one of your group has a userid on BITNET or EARNET, I can send you the whole file. You might want then to have Morrison put you on his distribution list; I find his reports very informative.

In any case, the warnings about BF3 counters certainly arouse suspicion of these results.

Accordingly, I would suggest that you look into the meteorology of 04/15-16/89. It would be very interesting to note the course of temperature and relative humidity over Saturday and early Sunday morning to see whether there might have been a problem with condensation on the wires of the counter.

We neglected to try to detect any sensitivity of the counter to ultrasound. I suppose a reasonable approach might be to pour sand from a height of 50 cm, so that the individual grains strike the case of the counter.

Looking toward the future, I feel that it is very important to put into operation at least one other counter, simultaneously and independently recorded. Surely, a burst of neutrons that saturates the black counter (as in Fig. 2) would provide simultaneous counts in one of your white counters at a similar distance. One does not even need to determine accurate relative timing of the bursts. The curve of Fig. 2 for the two counters should track one another.

I am by no means sure that the bursts of Fig. 2 are caused by neutrons. As I indicated, the neutrons should emerge with an exponential tail in time from the moderator, so that one would have to invoke not only multiple neutrons but also sever saturation of the detector in order to prevent counts emerging after each burst at a decreasing rate over a multiple of 60 microseconds.

The databook for the counter does not have a schematic diagram of the preamplifier. I am sure that you either have one or will obtain one from the manufacturer. Could you send me a copy?

But if one claims those bursts are neutrons, then it seems to me that you are obligated to determine experimentally the response of the counter to relevant neutrons and to neutron bursts. I suggested that you put a plastic scintillator near the black counter and illuminate it with fast neutrons from a Pu-Be source. If the pulse from the scintillation

counter opens a gate, in about one percent of the cases you will see a delayed count from the BF3 counter, and a plot of these delayed coincidences vs. time should be the curve rising to a maximum in 10 or 20 microseconds (as discussed in the databook) and falling eventually with an exponential tail. Thus you will have experimental proof of the single-neutron response curve of the BF3 counter.

The response of the preamplifier could be determined by using a pulse generator to detect a burst of "neutrons", and feeding that to the terminal joining the preamplifier to the counter tube. I hope, also, that your microton experiment works.

I would be pleased to hear from you of the results of your efforts to repeat your very interesting experiment.

Thanks very much for providing more details of the results of the experiment of G. Mazzone and M. Vittori, which were just sketched in Santa Fe by Scaramuzzi. I have informed Jim Ziegler, and I hope very much that he will repeat this experiment quickly. I will let you know the results.

You asked me about U.S. funding of conventional fusion programs, and I mentioned a recent article. Here it is from the International Herald Tribune of 06/09/89.

Very best regards.

Sincerely yours,

Richard L. Garwin

Encl:
06/09/89 "Fusion Furor Has an Impact on 'Hot' World," by P. Kemezis in International Herald Tribune. (060989..PK)

RLG:jah:163%MM:061289..MM

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

June 14, 1989
(Via FAX to 9-(409) 845-4205)

Professor John Bockris
Texas A&M University
Department of Chemistry
College Station, TX 77843-3255

Dear Professor Bockris:

Thank you very much for the draft paper sent 06/09/89, "Observation of Tritium Production ..."

I have two major problems with this paper. I will refer to the text by page and decimal fraction of a page.

Page 2.9 You say that approximately 10**10 atoms of tritium are produced per second, neglecting losses to the gas phase. Of course, in the mechanism that you eventually cite, we know very well that a number of tritium atoms per second produced would be equal to the number of neutrons produced, and you mention (page 3.2) "50 neutrons per minute", but where are the rest of the 10 billion neutrons per minute that you should be producing if your tritium is due to fusion?

So that is my first problem-- that not only are you assuming fusion under some very strange circumstances, you are assuming fusion in a bombarding energy range that has been well explored (I designed and participated in an experiment in 1951 to measure exactly this cross-section), but also the mechanism that you cite will not work.

Page 4.1 Here you say "Here, at the low radius of curvature tips, local electric fields of approximately 10**10 V/cm may bring the D+ ion in transfer to an energy of 10 keV sufficient to fuse with an absorbed D+ on the electrode surface." Well, even though one may get such very high electric fields, the <u>energy</u> that a charge can reach is simply the electric field times the charge times the distance over which the electric field operates. And this distance is <u>very</u> small. In fact, the distance is limited by the applied voltage, which is 10 volts rather than 10,000!

Therefore, no matter how low the radius of curvature of the tip, one can still not by this mechanism obtain deuterons of energy in eV greater than the voltage applied to the cell.

That has always been the problem, and it remains the problem.

Please let me know your views on these remarks.

Very best regards.

Sincerely yours,

Richard L. Garwin
Forwarded in his absence

RLG:jtml:165%JB:061489..JB

8A

15 JUN 89 9:32

-R.L. GARWIN-

TEXAS A&M UNIVERSITY

DEPARTMENT OF CHEMISTRY
COLLEGE STATION, TEXAS 77843-3255

PLEASE DELIVER THE FOLLOWING PAGE(S) TO:
Dr. Richard L. Garwin
IBM Research Division

FROM: Dr. J. O'M. Bockris

DATE: 6/14/89

TIME: 3:30 pm, cst

TOTAL NUMBER OF PAGES INCLUDING THIS PAGE IS 4

OUR TELECOPY NUMBER IS (409) 845-4205.
CONFIRMATION NUMBER IS (409) 845-5335.

THANK YOU!

061489JOMB

TEXAS A&M UNIVERSITY
DEPARTMENT OF CHEMISTRY
COLLEGE STATION, TEXAS 77843-3255

June 14, 1989

Dr. Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598

Dear Dr. Garwin,

Thank you for your faxed notes on a draft of our tritium paper. My answer:

(1) Experimental Results in scientific measurements have a certain probability of being "real" (i.e.is acceptable to the majority of scientists believing in the paradigms of the time). The solutions from the electrode which gave tritium were analyzed by two independent methods (different buildings, independent workers) at Texas A&M; three governmental labs at two of which extremely experienced tritium analysts did the work and two private organizations, at one of which an extremely experienced tritium analyst did the work. All results agreed to ±2%. Under such circumstances, the result is effectively certain, - certain, that it, that tritium to the stated degree was in the solutions given for analysis.

We have examined multiply the probability for it being there illicitly. We conclude that the wilful injection in secret of tritium by some person is the only possibility. However, the tritium-time relation, - particularly its final value, <u>does</u> correspond to the amount which would be expected to be there had an electrodo been producing energy by means of the process:

$$D + D \rightarrow T + H + 4.02 \text{ Mev.}$$

for the time of the electrolysis. I doubt if anyone who handled the measurements knew enough of the physical chemistry of solution - gas-equilibria to do the calculations necessary to put in the right amounts.

If these statements convince you that in 7 out of 11 electrodes, tritium was produced during the electrolysis of D_2O (LiOD) then the objections you make (both which come from theory) can be set aside without argument. The assumptions to the theory used must be inapplicable to the circumstances.

However, there would be the following to comment.

(1) Branching ratios: the neutron production by all the people who have measured neutrons from electrodes (and this ("The Jones Effect") now seems established), are all far too small to produce heat. Because of the rare character of their appearance, and the fact that they seem to turn up after very long electrolysis, I am at present in favor of the idea that they occur in a Kiliow mechanism, - cracking due to embrittlement (A field in which I have much experience). There is a sonic technique whereby such cracking can

be heard and we plan to listen for cracks and attempt to correlate such cracks with neutron emmission.

Measurement of tritium to the same order as that found here has also been observed by Grozzi (Rome) and by Schoesser and Wollingford (Gainesville). If one accepts it as established (and it is much less firm than the neutrons), then it seems to provide evidence that fusion does occur (whether the source of the heat or not) and that the branching ratio in solid state confinement differes from that in plasma. Dr. Guang Hai Lin, a physicist working with me, has a model which seems to him qualitatively to rationalize this difference.

The vital point in your letter concerns my attempt to rationalize fusion in the electrical double layer. My attention was first drawn to this idea was a telephone conversation with Csikei (Hungary), who reported in early April that he was getting neutrons but that they faded after an hour or two and could be brought back by cleaning the electrode surface.

At first I didn't think Csikei's confident statement that the potential difference in the double layer could give energies sufficient for fusion was credible. However, more recently I have argued as follows: order of magnitude of temperature for D + D fusion = 10^8°K. Corresponding energy in ev = 10^4ev. When a deuteron in the Helmholtz layer at an electrode discharges, (deuteron transfer) one can see it (I simpify) as travelling about 1Å though a field of 10^8 volts cm^{-1} and having, therefore, at the point of impact with an adsorbed deuteron ion on the electrode surface an energy of 1 ev. If, therefore, it is possible to find heterogeneous points in the double layer where the local electrid field is 10^{11} volts cm^{-1}, the deuterons arriving there will have an impact energy of 10^3 ev. (note, not 10^4 ev).

What is the probability of this? It is possible to show that

$$\frac{\text{field at promitary of radius } r}{\text{field at surface of radius } R} = \frac{R}{r}.$$

Now dendrites have tip radii of10^{-5}-10^{-6} cm taking R as ~ 1mm (our electrodes), one easily obtains a 10^4 magnifying factor, more than enough.

There is much more to say, - too much for the letter. In recent years, Henderson (IBM, Almaden) and others have shown that the metal electron gas extends out a few Å and would envelop the region in which the D^+ -$D^{\delta+}$ impact occured. Screening! I cannot here develop the model for $D^{\delta+}$ (the adsorbed deuteron) but I think I can show its reduced charge (charge transfer to the metal) will also ease repulsion.

Thus, one begins to see some rational model. I have written these words in the interests of collegial co-operation and discussion. I don't believe their substance should be included in my NOTE. This is intended as a brief communication and its intention is to make credible the massive production ot tritium at some electrdodes. I don't believe more can be expected at this time. I am, after all, not a nuclear theorist working in solid state

confinement. Criticisms of my Note, I believe, should try to shake the validity of the facts it reports.

I stress that only 7 out of 11 electrodes gave tritium. Further, an excess heat producing electrode (Fleischmann-Pons Effect) gave an increase of tritium which could be explained in terms of isotopic enrichment due to electrolysis. Conversely, the tritium reported in the paper is enough (counting the gas-phase amount) to be consistent with heat production through $D + D \rightarrow T + H$ (although we do not know if the tritium producing electrodes produced heat).

Sincerely,

J. O'M. Bockris

P.S. I don't think these matters will be resolved for a year or two. The time of charging of electrodes which have radii of curvature sufficient to give heat to be measured by most calorimeters available is 1-3 months. Few valid experiments have been done (those at SRI (McKubre) seems to avoid all the criticism I can bring to most).

After the facts are known, and these nuclear particles connected with the heat (if such a connection is made), -this is the time to think about fusion and theorize. To be frank with you, theorists can predict where the horse is only after they have been told it has left the stable.

.qq
&EXIT

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If you have any questions, contact your manager.

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Date: Mon, 26 Jun 89 10:42 EDT
From: <JBIGELEI@SBCCMAIL>
Subject: Huggins Visit
To: rlg2@yktvmv
X-Original-To: rlg2@yktvmv.bitnet

State University of New York at Stony Brook
Stony Brook, NY 11794-3400

Jacob Bigeleisen
Professor
Chemistry
516-632-7905
26-Jun-1989 10:30am EDT

FROM: JBIGELEISEN

TO: Remote Addressee (_RLG2@YKTVMV.BITNET)
TO: Remote Addressee (_HOFFMAN@LBL.GOV)
TO: Remote Addressee (_HUIZENGA@UORCHEM.BITNET)
TO: Remote Addressee (_KOONIN@CALTECH.BITNET)

SUBJECT: Huggins Visit

I am bringing to your attention something you should be aware of when you visit Huggins. To the best of my recollection, Huggins told me in Santa Fe (at the LANL reception) that when he connects two identical cells in series, one filled with H2O and one filled with D2O, the one filled with D2O runs hotter. I neglected to ask him whether he measured the IR drop individually across each cell. In the CERN Newsletter, A:061989.DRM (20 June 1989) distributed by Goodwin from copy of Morrison to Garwin, there is mention of other experiments which show that D2O cells run hotter than H2O cells. This is simply a chemical isotope effect associated with the lower zero point energies of D2O, OD- and Li(D2O)4+ compared with the respective H2O species. As as result of these lower zero point energies all the deuterium species are bound more tightly than the H2O species. This leads to a lower specific conductance of LiOD in D2O than LiOH in H2O. The D2O cell has a higher resistance and a larger IR drop and larger heat generation. Jacob Bigeleisen

Please distribute to NRAB Panel 07/11/89 (Done)

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

July 10, 1989
(Via FAX to 9-415-725-4034)

Professor Robert A. Huggins
Peterson Lab (Bldg 550), Room 550-I
Stanford University
Stanford, CA 94305

Dear Bob,

Thanks very much for your prompt reply of 07/07/89 and particularly for the last page of 06/06/89 with details on the cell running since 05/18/89. This is very much the kind of data that I want, but even it does not get back quite so close to the experiment as is necessary.

First, I was pleased to have the indication of "cell resistance" for the hydrogen cell and deuterium cell that you included in your letter. Operationally, is this "cell resistance" simply the voltage divided by the current at the indicated power?

Now for the data sheet of 06/06/89: Each of these major entries, as I understand it, is for a cell that has achieved a certain steady temperature at the indicated power of (say) 6.008 W. I would like to see the microvolts of the thermocouple in the bath and in the cell, which are presumably converted to temperature.

Then you add 2 W, 4 W, or 6 W and obtain the three K values indicated. If I am wrong, please let me know.

Using these three K values, you then use a single temperature difference between the cell operating at 6.008 W, and the bath, you should obtain the ratio of output power to input power shown in the last three columns.

I have verified that the "Ave" in all cases has properly computed the three "Value" entries in the previous column. But I don't understand the "% Spread" numbers, and my bewilderment is of two types.

First, this is certainly not the overall linear spread. But neither is it the standard deviation.

My second problem comes in the comparison of "% Spread" in the last column with that in the fourth column. From the

figures given, these should be identical, and they are not. How are they calculated?

Beyond that, I have specific questions as to how the heater power is stabilized during the 20 minutes to 0 min required for the temperature to stabilize, and how the voltage and the current are measured for the heater and for the electrolysis. Do you have multiple digital voltmeters? Or do you switch the single digital voltmeter from one reading to another?

Finally, for this letter, with the indicated K, even when the cell is running at 10 W plus the 6 W of calibrating power, the temperature rise should be only about 11 C above the bath. Why were we seeing voltages corresponding to temperatures like 55-60 C when we visited your Lab?

Incidentally, is there a measurement of the electrolysis voltage and current while you have the calibrating power applied? For instance, 6 W of calibrating power should raise the bath temperature about 4 C, which might correspond to a reduction of resistance of perhaps 4 percent, and an increase in electrolytic power by about the same amount during the calibration. Thus, the temperature rise that you assume due to your 6 W of calibrating power would instead be due to 6.4 W of power. The actual K would thus be less than indicated, and the power out from the cell greater than indicated.

In any case, if you no longer use strip chart recordings, how do you account for varying current or voltage during your measurements?

Specifically, could I have the page of primary data (micro voltages vs. time) corresponding to the very interesting page of 06/06/89?

Please send this to me via FAX on Monday c/o Bill Woodard at (202) 586-3119. I will be in Seattle on Monday, but will be in Washington all day Tuesday for a meeting of our ERAB Cold Nuclear Fusion Advisory Panel.

Thank you very much for your cooperation.

Sincerely yours,

Richard L. Garwin
Forwarded in his absence

cc:
W.L. Woodard, DOE (Via FAX to 9-202-586-3119).

RLG:jah:191%RAH:071089.RAH

07/08/89

Re Huggins FAI of 07-07-89.

1) Rlb sent to Woodard for use at ovn ntg 07/11/89.

2) Page #4. assume "calibration curves are for applied of 2, 4, 6 watts;

$$0.682 = \frac{\Delta T \Delta P}{\Delta P} = \frac{\Delta T_2}{2}; \quad \Delta T_2 = 2 \times 0.682 = 1.364\ ^\circ C$$

but this "ΔT" must have been obtained by a pre-cal T_i. (created from the thermistor table / subtracted from the Cal T_f (created from voltage to T_f).

Then, the both must have been subtracted, and the electrolyte pump resumed to ensure that it has not heated or will when one adds the calibrating power.

** Assume $\frac{dT}{dtime} = -0.01\ sec^{-1}$; assume input electrolyte voltage stabilized at ~~6 volts~~ 10 volts, heated 0.6 deg (6 watts) $\rightarrow 6 + 6 \times 1.36 \times 10^{-2} = 6.08$ W.

3.1

STANFORD UNIVERSITY

FAX

To aid in prompt delivery, the following information sheet will precede your FAX message.

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To:

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From:

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General Information

Date 7/7/1989 Time 14 00

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Dr. Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
FAX (619) 455 - 3943

July 7, 1989

Dear Dick:

I received your FAX just as we are on the way out the door to embark upon a trip for several days.

1. I am afraid that we do not have time at this moment to compile and plot the data you asked for on the temperature dependence of the cell calibration constant from our many experiments.

2. With regard to the electrolyte resistance issue that you raised in your second paragraph: you should recognize that the electrolyte resistance, along with the interfacial impedances, contributes to the sum of irreversible ohmic terms that are included in both the energy supplied and the energy released by the cell in this type of experiment.

The energy supplied = $E\,I\,t = (E_{rev} + E_{irrev.})\,I\,t$.

The energy released = $(E_{rev} + E_{irrev.} + E_{excess} - E_{tn})\,I\,t$

Therefore, since the irreversible terms are included in both the energy supplied and energy released, they will not affect the difference, which is a direct measure of the enthalpy change.

3. Our measurements on the distribution of the overall potential between the two interfaces and the electrolyte, which involve the use of a reference electrode, are very sparse to date, and were not under a wide range of conditions. We shall be undertaking more of these in the near future.

To give some indication of the ranges of total cell resistance in some of our experiments, from data on May 19 and 20, 1989, we find

$P = E \cdot I = E^2/R$

For a hydrogen cell:

Power In	Cell Resistance	E	I	$(E-1.48)/I$
3.37 watts	16.2 ohms	7.39	0.456	12.96
8.512	11.865	10.05	0.847	10.12

For a deuterium cell:

Power In	Cell Resistance	E	I	$(E-1.53)/I$
3.435	21.683	8.63	0.398	17.84
8.569	15.948	11.69	0.733	13.86

In addition, there were some questions about values of power, calibration constants, output versus input power, spreads, etc. Perhaps the enclosed summary of data from our experiment on June 6, 1989, made on a deuterium cell that had been running continuously since May 18th, would be illustrative.

Please note that these calculations are based upon the total energy in, without subtracting the thermoneutral voltage, so that the Power Out to Power In ratios are of the most conservative type.

Also note that, since as mentioned above, the irreversible (Joule) terms occur in both the power in and power out values, one could increase the ratio of P_{out} to P_{in} by reducing those parameters. This is one direction to consider if one wishes to enhance the observed effects.

Bob Huggins

Stanford

R. A. HUGGINS

June 6, 1989

Stanford Experiment
D-Pd cell running since May 18, 1989

60689D5 / Pd 7

Power	ΔT/P Value	ΔT/P Ave	ΔT/P % Spread	Pout/Pin Value	Pout/Pin Ave	Pout/Pin % Spread
6.008	0.682 (0.038) 0.644 (0.019) 0.650 0.625		± 3.6 S.D. ~~± 3.8~~	1.075 1.138 1.173	1.129	3.6 S.D. ± 4.8
7.164	0.700 (0.03) 0.668 (0.02) 0.672 0.649		± 3.1 S.D. ± 3.5	1.047 1.098 1.129	1.091	± 3.1 S.D. ± 4.1
8.058	0.675 (−0.016) 0.691 0.691 0.708 (−0.017)		1.9 S.D. ± 2.3	1.090 1.063 1.038	1.064	± 2.4
9.029	0.721 (0.046) 0.677 0.699 0.700 (−0.003)		± 2.6 S.D. ± 3.1	1.052 1.119 1.083	1.085	± 3.0
10.464	0.684 (−0.01) 0.704 0.696 0.699 (−0.005)		± 1.2 ± 1.7	1.096 1.065 1.072	1.078	± 1.2

↑
Calibration Constant

↑

$\frac{\text{Power Out}}{\text{Power IN}} = \frac{K(\Delta T)}{EI}$

This does not include the additional power out related to the chemical energy in the gaseous products. The thermoneutral voltage was not subtracted, so this is excess power, even by the most conservative calculation.

1,405 °C

7,814 °C. ΔT?

Where are the primary data? Raw voltage & current measured?

1) What are the T_p & T_B for the various powers?

2) If all 3 calculations at a given input power -- e.g., 8.058 W, are for the same $T_p - T_B$, how is the "% spread" different for the "% spread" in $\frac{\Delta T}{P}$?

At P = 6.58 W -- $\bar{K} = 0.661$

At P = 3.41 W K → 0.625 "/w

At P = 9.75 W $\bar{K} = 0.697$